

*Desk* JUL 31 1950  
PERIODICAL ROOM  
GENERAL LIBRARY  
UNIV. OF MICH.

VOL. LXXI, 3

WHOLE NO. 283

# AMERICAN JOURNAL OF PHILOLOGY

Founded by B. L. GILDERSLEEVE

Edited by  
**HENRY T. ROWELL**

**KEMP MALONE, BENJAMIN D. MERITT**

**JAMES H. OLIVER**

**EVELYN H. CLIFT: Secretary**

Honorary Editor  
**DAVID M. ROBINSON**

JULY, 1950

BALTIMORE 18, MARYLAND  
THE JOHNS HOPKINS PRESS

## CONTENTS.

	PAGE
Another Fifteenth-Century Manuscript of the <i>Germania</i> . By DOROTHY M. ROBATHAN, -	225
"Pisidian." By ROBERT SHAFER, -	239
Some Epigrams by Leonidas of Tarentum. By CHRISTOPHER M. DAWSON, -	271
Five Minor Elucidations in Seneca's <i>Moral Epistles</i> . By WILLIAM HARDY ALEXANDER, -	285
The Meaning of <i>oīnw</i> . By P. T. STEVENS, -	290
Hadrian's Precedent, The Alleged Initiation of Philip II. By JAMES H. OLIVER, -	295
Observations on Thucydides. By GEORGE A. PAPANTONIOU, -	299
Announcement: Prosopography of the Later Roman Empire, -	305
<b>REVIEWS:</b> -	<b>306</b>

*van der Valk's* Textual Criticism of the *Odyssey* (G. M. BOLLING).—*Lepper's* Trajan's Parthian War (ALFRED R. BELLINGER).—*Solmsen's* Hesiod and Aeschylus (WILLIAM C. GREENE).—*Karo's* Greek Personality in Archaic Sculpture (MARGARETE BIEBER).—*Lowe's* Codices Latini Antiquiores. A Palaeographical Guide to Latin Manuscripts Prior to the Ninth Century. Part IV: Italy: Perugia—Verona (LUDWIG BIELER).—*Marmorale's* La Questione Petroniana (†ELI E. BURRISS).—*Buck's* A Dictionary of Selected Synonyms in the Principal Indo-European Languages (JAMES W. POULTNEY).—*Saint-Denis' Le vocabulaire des animaux marins en Latin classique* (ALFRED C. ANDREWS).

---

The American Journal of Philology is open to original communications in all departments of philology, and especially in the field of Greek and Roman studies. It is published quarterly. Four numbers constitute a volume, one volume each year. Subscription price, \$5.00 a year, payable in advance (foreign postage 25 cents extra); single numbers, \$1.50 each.

Articles intended for publication in the Journal, books for review, and other editorial communications should be addressed to the editor, Henry T. Rowell; proof should be returned to the secretary, Evelyn H. Clift, The Johns Hopkins University, Baltimore 18, Maryland.

Contributors are entitled to receive twenty-five copies of their respective contributions free of charge. Additional copies will be supplied at cost. Subscriptions, remittances, and business communications should be sent to

THE JOHNS HOPKINS PRESS, Baltimore 18, Md.

---

The contents of the American Journal of Philology are indexed regularly in the International Index to Periodicals.

Entered as second-class matter October 16, 1911, at the postoffice at Baltimore, Maryland, under the Act of March 3, 1879.

Acceptance for mailing at special rate of postage provided for in Section 1103, Act of October 3, 1917. Authorized on July 3, 1918.

# AMERICAN JOURNAL OF PHILOLOGY

---

VOL. LXXI, 3

WHOLE No. 283

---

## ANOTHER FIFTEENTH-CENTURY MANUSCRIPT OF THE *GERMANIA*.

In 1935 when a critical edition of the *Germania* of Tacitus appeared, based upon all the known manuscripts of that work, its editor predicted that undoubtedly other manuscripts would come to light in European libraries, "but barring some rare good fortune that would restore to us the Hersfeld codex itself or one of its immediate apographs, I think it hardly probable that future discoveries will add greatly to our knowledge of the text of the *Germania*."<sup>1</sup> Unfortunately, in the case of the codex which I came upon accidentally in the Biblioteca Vaticana in the spring of 1949, this prophecy is all too true!<sup>2</sup> Ottobonianus 1434 is neither the Hersfeld manuscript nor a direct copy of it, and it does not add to the slight number of manuscripts of the X family, upon which Robinson's text is primarily based. Nevertheless, I believe that it is of interest for the history of the transmission of the text, not only of the *Germania*, but of the *Dialogus de Oratoribus* and of the *De Grammaticis et Rhetoribus* of Suetonius, which it also contains.

An unusual feature of the classification of manuscripts in Robinson's edition of the *Germania* is the assignment of only three manuscripts to the X branch of the lost copies of the

<sup>1</sup> Rodney P. Robinson, *The Germania of Tacitus* (Middletown, Conn., 1935), p. 79, n. 1.

<sup>2</sup> Ottobonianus 1434 does not appear in the Index to the Catalogue of this collection under *Tacitus*, presumably because neither the names of the authors nor the titles of the works contained in it appear in the codex itself.

Hersfeld codex (from which he believes that all extant manuscripts are derived), while the remaining twenty-eight (including two early editions) represent the Y family. This latter branch is in turn divided into two groups, of which the one called *a* has two divisions. Into one of these ( $\sigma$ ) were introduced X readings as marginal or interlinear variants. The children of  $\sigma$ , according to Robinson's stemma, are N (Naples Bib. Naz. IV. C. 21),  $\Delta$  (Vat. Lat. 4498) and I (Vat. Lat. 1518), of which I preserves the Y readings to a greater extent than its two brothers, which more frequently incorporated X variants into their texts.<sup>3</sup> I's reputation, however, is not so unblemished as this fact might suggest. Described by Robinson as "by far the most corrupt of its group,"<sup>4</sup> by an early editor of the *Dialogus* as written "tamquam per somnum,"<sup>5</sup> by a later editor of the same work as "die mechanische Abschrift eines unwissenden Schreibers, der seine Vorlage nur mangelhaft lesen konnte oder dem Sinne des Textes gleichgültig gegenuberstand,"<sup>6</sup> one might wonder what contribution to the history of the text tradition such an unreliable codex could make. For a reply to this query we must recall that for all three of the works which we have mentioned, there are no extant manuscripts earlier than the fifteenth century and that anything that may throw light upon the text of the lost ancestors of these codices is not to be scorned. Thus Robinson pays tribute to I as a manuscript which "in some cases appears to be the only manuscript of the  $\sigma$  branch showing corruptions originating in Y,"<sup>7</sup> and again, "it is quite possible according to my classification of manuscripts for I alone to give the readings of *a*, since all manuscripts of this group (though I very rarely) show conflated

<sup>3</sup> Robinson, *op. cit.*, pp. 91-93. All references to the *Germania* in this paper are to Robinson's edition; to the *Dialogus* to the edition of A. Gudeman (Berlin, 1914); to the *De Grammaticis et Rhetoribus* to page and line of R. P. Robinson's edition (Paris, 1925). Since Vat. 1518 is Robinson's I for the *Germania* and *De Grammaticis*, but C for some earlier editions, and D for the *Dialogus* in Gudeman's edition, I use Vat. throughout this paper when comparing it with Ott. (Ottobonianus 1434).

<sup>4</sup> Robinson, *Germ.*, p. 190.

<sup>5</sup> Adolf Michaelis, *Dialogus de Oratoribus* (Leipzig, 1868), p. xiii.

<sup>6</sup> Gudeman, *op. cit.*, p. 120.

<sup>7</sup> Robinson, *op. cit.*, p. 190.

texts."<sup>8</sup> This distinction, formerly accorded to I alone, must now be shared with Ott. 1434.

The close relationship between these codices is first suggested by the identity of contents: Porphyrio on Horace, "Cornutus" on Persius, and the *De Grammaticis*, *Dialogus de Oratoribus*, and *Germania* all in the same order.<sup>9</sup> Closer scrutiny reveals striking agreements, such as repetition of eight words in *Germ.* 9, 7 with no expunging marks; omission of eleven words in *Germ.* 37, 21 and of seven lines in *Germ.* 46, 4-11, both *sine spatio relicto*<sup>10</sup> and for the well-known lengthy lacuna in *Dial.* 35 a space of twelve and a half lines, which is certainly significant.<sup>11</sup>

In view of these arresting similarities, our first supposition is that one of these manuscripts is a direct copy of the other. With this thought in mind let us examine them side by side as I was able to do in the Biblioteca Vaticana. We note, in the first place, that both manuscripts suggest de luxe editions with their large parchment folios and wide margins.<sup>12</sup> In Vat. the illumi-

<sup>8</sup> *Ibid.*, p. 117, n. 4.

<sup>9</sup> Vat.: f. 1 Porphyrio, f. 110 "Cornutus"; f. 166 v *Gram.*; f. 174 dup. v *Dial.*; f. 189 dup. v *Germ.* Ott.: f. 1 Porphyrio; f. 124 "Cornutus"; f. 174 v *Gram.*; f. 182 v *Dial.*; f. 197 *Germ.* According to Robinson, Porphyrio originally belonged to another manuscript (*Germ.* p. 83). This statement is based upon the fact that the parchment of the Porphyrio is coarser and heavier; it is ruled with a plummet on both sides, while the rest of the codex, which was written by a single scribe, was ruled with a hard point usually on the flesh side; in general the Porphyrio has more lines to the page than the other authors. (From a personal letter from Prof. Robinson.) On the other hand, we may note that there are several hands represented in the Porphyrio alone, and that the number of lines in that work varies from 35 (f. 1), 31 (f. 39), 33 (f. 108), while the rest of the manuscript, as Robinson observed, seems to have 31 lines uniformly. I find no indication in the arrangement of the quinions or in the numbering of the folios to support the theory that the first eleven quinions originally belonged to a separate codex. Nor does Nogara (*Cat. Cod. Vat. Lat.*, III) describe it as a composite manuscript.

<sup>10</sup> The omission of 13 words in *Germ.* 6, 16 may not be significant, since the recurrence of the word *numerus* may have caused both scribes to make the same error independently.

<sup>11</sup> The length of the passage omitted is variously reported in other codices but none indicates this number of lines. Cf. Gudeman's apparatus.

<sup>12</sup> Vat. has 200 folios (280 x 180 mm.); Ott. has 209 (280 x 210 mm.),

nation is limited to two initials and a border on the first folio, whereas Ott.'s initial of gold set into a richly decorated background, which we admire on folio number one, is repeated on a smaller scale, not only at the beginning of each major work, but within the Porphyrio at the beginning of each work of Horace. The exquisitely designed and beautifully colored border that encloses three margins on the first folio has inserted into it a colored *stemma*, which, unfortunately, I have been unable to trace.<sup>13</sup>

Both manuscripts were obviously written in the latter part of the fifteenth century. The script of Ott. is more cursive in character and, in spite of the fact that it has fewer abbreviations than Vat., it is more difficult to read.<sup>14</sup> This codex was written by one scribe, in contrast to Vat., where we find a number of different hands in the Porphyrio alone. There are no marginal or interlinear notes and all corrections seem to have been made by the scribe himself. In Vat., however, I distinguish two hands in the text besides a third that added one quotation in the margin of folio 170 v.<sup>15</sup> Both codices have frequent lacunae where their scribes were unable to make out the words of the exemplar, and Ott. has additional spaces where Vat. has Greek words in the text.<sup>16</sup>

consisting of 21 quinions, the last of which has only 9 folios. The last folio is incorrectly numbered 206, since folios 19 and 183 were omitted in the numbering and there are two folios numbered 125. Neither the titles of the works nor names of the authors appear anywhere in the codex. At the end of the last folio the scribe wrote *Finis la. deo*, where Vat. has *θελος*.

<sup>13</sup> Although some of the color is rubbed off, it is possible to identify an eagle in the upper part of the shield, of which the outline is composed of nine broken arcs. The *Schedario Araldico* in the Vatican (by Marchese Luigi Rangoni Machiavelli), which contains several volumes of stemmata with eagles, failed to disclose this particular one.

<sup>14</sup> The script has a definite left to right slant. Letters *r*, *t*, and *c* are much alike; the 2 form of the *et* ligature, as well as *æ*, is found. Other ligatures which occur are *st*, *ct*, *N*, not only at the end of a word, but occasionally in the middle, e.g. f. 175 *Gram.* 3, 7 *interpretabanur*; f. 187 v *Dial.* 17, 18 *volumnas*.

<sup>15</sup> Cf. pp. 232-3 below for description of *m<sup>2</sup>* and pp. 235-6 for discussion of the Cicero quotation.

<sup>16</sup> Almost every folio of the Porphyrio has some lacunae where Vat. has Greek words. Cf. also *Germ.* 3, 13 Ott. f. 197 v *sp. rel. 9-10 litt.* Vat. 189 dup. v. *ασκιγουριον*.

As we study these manuscripts more closely, we find further evidence of their kinship from a formidable list of agreements in errors in all three works,<sup>17</sup> and we are also able to establish their close relationship within the  $\sigma$  family. Robinson's detailed analysis of the extant codices of the *Germania* includes a list of thirty-three cases where members of the  $\sigma$  branch of  $\alpha$  are not always in agreement with one another.<sup>18</sup> In every case where I (Vat.) is at variance with N  $\Delta$  or with either one alone, we find Ott. supporting I, with the following exception: *Germ.* 28, 18 Treboci Vat. Troboci Ott.

In spite of striking agreement in omissions and errors, a word for word comparison of these two codices makes it clear that neither one could have been copied from the other. We note, for example, lacunae in Ott. where the readings in Vat. are legible, e. g. *Gram.* 37, 6 senior; *Dial.* 3, 1 materni, 41, 9 ars; *Germ.* 44, 8 differt. Furthermore, Ott.'s text includes many errors that could not possibly have arisen from misreading of the script of Vat. Collation of the two manuscripts with the published texts shows that in the *Germania* alone Ott. has 58 errors not found in Vat., while in the *Dialogus* there are 57 such readings, and in the *De Grammaticis* 26. Surely in a contest of corruptions Vat. must now share the honors with the newest entrant into the field! Conversely, we note that Ott. sometimes has the correct form where Vat. is in error, e. g. *Dial.* 1, 13 dicta Ott. dictam Vat.; 7, 9 alio Ott. animo Vat.; 21, 19 eloquentia Ott. eloquentiam Vat.; *Gram.* 27, 6 nota Ott. vota Vat.; 33, 6 centuriata Ott. conturiata Vat.; 38, 9 senatorium Ott. sonatorium Vat.

If neither Ott. nor Vat. is a copy of the other, two possibilities

<sup>17</sup> Publication of the complete list does not seem feasible in a paper of this length. It is hoped that the examples cited in the discussion which follows will be sufficient evidence of this phase of the relationship between the two manuscripts.

<sup>18</sup> Pp. 188-189. Throughout this paper my conclusions are based primarily upon the *Germania*. Readings from the *Dial.* and *Gram.* are introduced as supplementary evidence. We must note, however, that for the latter work  $\Delta$  does not belong to the same manuscript tradition, whereas G (Gudianus 93) has been recognized by Robinson as a close relative of N and I (Vat.). Cf. Robinson, *De Fragmenti Suetoniani de Grammaticis et Rhetoribus Codicum Nexus et Fide* (Univ. of Illinois Press, 1922), p. 109; *Germania*, p. 190, n. 1.

suggest themselves: (1) that there was an intervening manuscript which introduced the new errors into the text of Ott. (2) that both are descended from the same parent, which not only contained many marginal and interlinear variants, but whose script, difficult to read and abounding in abbreviations, offered a fertile field for new errors. Although the former theory at first glance seems possible,<sup>19</sup> a study of the errors peculiar to Ott. and a consideration of corrections in both codices support the latter hypothesis, for the nature of the mistakes suggests that they originated in an ancestor rather than in a descendant of Vat.

Robinson, in discussing the nature of the archetype of the extant manuscripts of the *Germania*, lists the types of "graphical" errors existing in the Hersfeld *Agricola*, with a view to determining the kinds of mistakes that we might have expected to find in the lost *Germania*.<sup>20</sup> It is interesting to note that many of Ott.'s distinctive errors fall into one of these classes. For example, Robinson points out that "the most common type of error in the Hersfeld *Agricola* consists in the omission of letters, syllables or even entire words."<sup>21</sup> The same observation could be made concerning the 141 errors in Ott. which do not appear in the text of Vat. I submit the following examples from each of the three works: *Germ.* 14, 11 liberalitate] libertate; 43, 19 feritati] feriati; 39, 2 stato] sta; 40, 15 amata] ama. *Dial.* 3, 11 maturare] mature; 3, 17 suffeceris] sufferis; 10, 4 recitationum] rationum; 25, 16 splendidior] splendior; 33, 14 exercitationes] exercitiones; 33, 19 obscuriora] obscura. *Gram.* 6, 11 melissus] melius; 19, 3 pupillum] pullum; 27, 2 scriberentur] scribentur; 49, 1 centumviriali] centuriari. Such errors point to incorrect expansion of abbreviations in an earlier manuscript rather than

<sup>19</sup> If, for example, the Porphyrio belonged to another manuscript, as Robinson believed, the natural supposition would be that Ott. was descended from Vat. after the composite manuscript had been put together. Also the recurrence of lacunae at identical spots in both manuscripts suggests this relationship, e.g., *Gram.* 22, 14 Vat. f. 171, Ott. f. 178; *Gram.* 38, 6 Vat. f. 173 v, Ott. f. 180 v; *Germ.* 15, 10 Vat. f. 191, Ott. f. 200.

<sup>20</sup> Pp. 45-56.

<sup>21</sup> *Ibid.*, p. 47.

to contractions in such a late manuscript as an apograph of Vat. Lat. 1518.<sup>22</sup>

Of errors arising in Ott. from confusion of words similar in appearance we may note: *Germ.* 36, 8 *rерum*] *reum*; 39, 1 *semnones*] *sermones*; *Dial.* 1, 5 *diserti*] *deserti*; 18, 2 *propriorem*] *propriorem*; *Gram.* 43, 4 *studiosissimus*] *stultissimus*. Interesting examples of transpositions of letters occur in *Germ.* 30, 4 *chattos*] *actos*; *Dial.* 1, 8 *iudiciis*] *divitiis*; *Gram.* 43, 1 *Plotius*] *Poltius*. Misreading of ligatures and abbreviations, as listed by Robinson, follows: *um* for *us* (or vice versa), *Germ.* 35, 1 *Hactenus*] *Hactenum*. *Dial.* 28, 25 *universum*] *universus*. *Gram.* 28, 10 *Alexandrinum*] *Alexandrinus*; *m* for *s* (or vice versa), *Gram.* 9, 4 *eosdem*] *eosdes*; 10, 10 *principem*] *principes* Ott. *principem* Ott.<sup>1</sup> In the confusion between *us* and *is* we find both scribes having difficulty at the same places: *Germ.* 21, 8 *apparatis*] *apparatus* Vat. Ott. *apparatis* Vat.<sup>2</sup>; *Gram.* 3, 15 *prolapsus*] *prolapsis* Vat. Ott. In *Gram.* 43, 4 Ott. read the final syllable correctly, but Vat.<sup>2</sup> changed an erroneous *studiosissimis* to *studiosissimus*. In *Germ.* 45, 2 we find the reverse situation, where Vat. has the correct *extremus* while Ott. copied *extremis*.

A fruitful source of error in the Hersfeld *Agricola* was confusion between certain letters of the alphabet. Here too we find examples among the corruptions peculiar to Ott.: *a* and *o*, *Germ.* 42, 9 *raro*] *roro*; 43, 6 *nahanarvalos*] *nahanarvolos* Ott. *nahanarvalos* Ott.<sup>1</sup>; <sup>23</sup> *b* and *d*, *Germ.* 14, 17 *sudore*] *suobre*; *Dial.* 33, 11 *adnuissent*] *abnuissent*; *c* and *t*, *r* and *t*, *Germ.* 45, 11 *securum*] *se tutum*; *Gram.* 29, 12 *togatorum*] *rogatorum*; *e* and *c*, *Dial.* 1, 1 *cur*] *eum* (*cū* > *eū*); *l* and *r*, *Germ.* 11, 16 *frameas*] *flameos*; *n* and *r*, *Gram.* 22, 8 *pompeiorum*] *pompeionum*; *p* and *t*, *Germ.* 21, 2 *chattorum*] *captorum*. Among the letters confused in the Hersfeld *Agricola* are *cl* for *d* and *p* for *t*. Although the following examples are not found in Ott. alone, they are significant for the study of the relationship between Vat., Ott., and their parent: *Dial.* 9, 16 *conducit*] *concludit* Vat. Ott. *conducit* Vat.<sup>2</sup>; *Gram.* 26, 2 *hemicyclium*] *hemicidium* Vat. Ott.; *Dial.* 21, 28 *Accium*] *Appium* Vat. Ott. *Attium* Vat.<sup>2</sup>

<sup>22</sup> It is unlikely that a scribe copying from Vat. would substitute the sort of abbreviations which would give rise to these errors.

<sup>23</sup> Note also *Gram.* 38, 8 *inieco*] *iniecta* Vat. Ott., *inieco* Vat.<sup>2</sup>

Confusion of letters not found in Robinson's lists is also indicated in the parent of Ott. and Vat. as follows: *e* and *o* *Germ.* 28, 18 *Treboci*] *Troboci* Ott.; 40, 7 *nerthum*] *northum* Ott. *nerthum* Ott.<sup>1</sup> *Dial.* 25, 6 *confesso*] *confosso* Ott. *Gram.* 38, 9 *senatorium*] *sonatorium*. Of such frequent occurrence that it cannot be disregarded is the substitution of *u* for *i* in Ott.: *Germ.* 40, 5 *muniuntur* *ex miniuntur* Ott.<sup>1</sup>; 38, 8 *imitatione*] *immutatione*. *Dial.* 18, 4 *impolliti*] *impolluti*; 25, 23 *soltos*] *solutos*; 39, 11 *indicit*] *inducit*. *Gram.* 28, 11 *immitatus*] *immutatus*. In all of these cases Vat. has the correct vowel.<sup>24</sup>

That the symbols for the prefixes *per* and *prae* gave rise to variant readings in the descendants of *σ* is clear from the following cases: *Dial.* 8, 9 *pervenerunt* Vat. *prevenerunt* Ott.; 19, 12 *percipitur* Vat. *precipitur* Ott.; 33, 18 *perceperis* Vat. *preceperis* Ott. In *Germ.* 11, 3 where the Y family is divided between *praettractentur* and *pertractentur* Ott. has *ptractantur*, whereas Vat. first had *ptractantur* but changed in the first hand to *ptractentur*. Robinson's suggestion that *per* was introduced into the text by confusion between the conventional symbol for *prae* (p̄) and an *r* abbreviation consisting of a wavy line (p̄̄) seems to find support in *Dial.* 6, 3 where Ott. has *hōis* (the usual abbreviation for *hominis*) while Vat. has the correct reading *horis*.<sup>25</sup> There are other examples, however, where unorthodox abbreviations have been preserved in Vat. and expanded into the normal but incorrect readings in Ott.: *Dial.* 10, 23 *voco*] *vō* Vat. *vero* Ott.; 31, 64 *misericordia*] *mā* Vat. *materia* Ott.; *Germ.* 33, 6 *Romanis*] *ro<sup>is</sup>* Vat. *rationis* Ott. Of interest too is the ludicrous *porcos* which Ott. has for *proconsulem*. It is not surprising to find that at this point Vat. has *procōs*. In some cases the abbreviation was unintelligible to Ott.'s scribe and he left a blank space e. g. *Germ.* 14, 7 *plerique* where Vat. wrote *plurique*, but later revised it to *populique*.

Mention has been made of the fact that Vat., unlike Ott., bears evidence of corrections in the second hand. Such revisions consist of one or two letters inserted with a fine pen in lighter

<sup>24</sup> The clue to the origin of this error is found in the last example, where Vat. has *immittatus*; obviously *immitt* > *immut*.

<sup>25</sup> Note in this connection *Germ.* 28, 8 where Ott. preserves the abbreviation *siḡtque*, which N Δ have expanded as *signatque* and Vat. as *significatque*.

ink than the text and sometimes accompanied by a hairline to indicate the correction. They are easily differentiated from the corrections made by the scribe himself, who usually changes the original letters, though occasionally he writes a superscript letter, with the heavier pen used in the text. Some examples of revision in the first hand are of interest because the original readings agree with the errors that remain uncorrected in Ott.: *Germ.* 13, 2 *armati*] *armate*; 14, 11 *principis*] *principes*; 24, 8 *iactu*] *actu*; 33, 8 *odium*] *tedium*; 38, 13 *compti*] *cumpti*; <sup>26</sup> 45, 13 *prosolita*] *prosilita*. *Dial.* 17, 4 *antiquis*] *antiquos*; 24, 10 *exprome*] *exprime*; 30, 17 *licet*] *dicit*; 37, 11 *quemquam*] *quamquam*. *Gram.* 3, 15 *prolapsus*] *prolapsis*; 38, 8 *inecto*] *inecta*; 40, 5 *appellationibus*] *appellations*.

In the above cases it would appear that either confusion between letters in the script of the parent or the presence of interlinear or marginal variants, caused the scribes of both Vat. and Ott. to make the same error in the first place. Vat.'s scribe, however, upon second thought adopted the alternate form. But in some other cases in Vat. the revision of an error with which Ott. agrees was made by the corrector: *Germ.* 7, 5 *neque*] *quoque*; 19, 12 *ne ulla*] *ne ullam*; 34, 3 *frisis*] *friscis*; *Dial.* 9, 16 *conducit*] *concludit*; 14, 19 *quod add.*; 14, 19 *eius*] *huius*; 20, 4 *Tullio*] *Tulio*; 21, 28 *Accium*] *Appium*; 33, 20 *concedet*] *contendet*; *Gram.* 23, 7 *eius*] *eis*; 35, 6 *edictum*] *editum*. When the correction takes the form of expunging dots, it is impossible to assign them with certainty e. g. *Germ.* 24, 1 *spectaculorum*; 24, 9 *de corpore contendat*. *Dial.* 31, 28 *debeat*. *Gram.* 14, 4 *syricus*.

An interesting example of a correction in Vat. in which both hands are represented is *Germ.* 2, 2 *adventibus*] *adetibus* Vat. *adetibus* Vat.<sup>1</sup> <sup>u</sup><sup>g</sup> *etibus* Vat.<sup>2</sup> Aside from the fact that the two superscript letters illustrate clearly the difference between the two types of corrections, this revision throws light upon the origin of the impossible *aductibus* found in Ott. One of the characteristics of the script of Vat. is the use of a superscript c-shaped abbreviating mark instead of the usual virgula. In

<sup>26</sup> Cf. also *Germ.* 18, 4 *ambiuntur*] *ambiontur* Vat. *ambiuntur* Ott. *ambiuntur* Ott.<sup>1</sup> *Gram.* 40, 13 *Brundisium*] *Brondisium* Vat. *Brundisium* Ott. *Brondisium* Ott.<sup>1</sup>

the case under discussion, if the *c* were over the *e* instead of squarely over the consonant, it might be taken for the abbreviating symbol. I believe that in the parent of Vat. and Ott. such an abbreviation was also used and that Ott.'s scribe mistook it for the letter *c*.

Further evidence of confusion that originated in the archetype of our manuscripts is found in such garbled versions as the following: *Germ.* 20, 16 propinquorum] propinquoris (is ex es) quorum Ott. propinquiores quorum Vat. propinquorum Vat.<sup>2</sup> *Dial.* 29, 16 inlecebris adulacionis] ille crebris adulacionibus Ott. ille crebris auditionibus Vat. audulationibus Vat.<sup>2</sup>; 31, 13 promptius] <sup>m</sup>promptius Ott. pmptius Vat.

Also significant for a study of the relationships between the parent and its descendants are some passages involving repetitions in both Vat. and Ott.:

*Germ.* 24, 1 genus spectaculorum unum atque in omni coetu  
idem]  
genus spectaculorum unum atque in omni coetu  
spectaculorum idem Vat. Ott.  
*exp.* spectaculorum Vat.<sup>2</sup>

*Germ.* 24, 9 iuvenior quamvis]  
iuvenior de corpore contendant (contendat Ott.)  
quamvis Vat. Ott.  
*exp.* de corpore contendant Vat.<sup>2</sup>

In both cases it seems that the scribe of the exemplar of Ott. and Vat., having discovered that he was repeating words which he had already written in preceding lines, failed to expunge them, or put in such faint dots that the copyist of Vat. saw them only after he had made the same mistake, while Ott.'s scribe failed to notice them at all. We have noted an instance in *Germ.* 9, 7 where both manuscripts repeat eight words with no expunging marks.

Another passage involving repetitions and corrections is more complicated:

*Dial.* 9, 20 animo cuiusquam beneficium sed clamore vagum et  
voces inanes et gaudium volucre laudavimus nuper  
ut miram et eximiam]  
animo cuiusquam volucre laudavimus beneficium sed  
clamore vagum et voces inanes et gaudium volucre

laudavimus nuper ad miram et eximiam. Vat.  
 animo cuiusquam laudavimus nuper ut miram et  
eximiam beneficium sed clamore vagum voces et  
 inanes et gaudium volucere laudavimus nuper ut  
 admiram eximiam. Ott.

In comparing these versions, we note some rather troublesome discrepancies. Ott. does not expunge *laudavimus* along with the other words which have been incorrectly inserted; it has only *ut* after *nuper* the first time, but *ut ad* the second time, and it omits *et* before *eximiam* only once; Vat. includes *volucere* twice, while Ott. did not make this mistake. The only explanation that I can suggest is as follows: the scribe of the archetype of our manuscripts skipped from *cuiusquam* to *laudavimus* and wrote *nuper ut miram et eximiam* before he discovered his error. He then inserted the missing words in such a way that only the last word appeared to the scribe of Vat. to be part of the text. But after he had written *volucere laudavimus* the copyist of Vat. saw the insertion mark and copied all the omissions, expunging the two words which he had previously copied. Ott.'s scribe, however, took the omitted words to be one of the numerous glosses which he was not interested in copying, but when he reached *eximiam* he realized that they were part of the text. In making his corrections, he failed to expunge the first *laudavimus* and instead of substituting *ut* for *ad*, as he did the first time, he included both. Vat.'s scribe, on the other hand, made the alternative substitution, which the second hand corrected. Even if this explanation is not completely satisfactory, it is hard to see how the situation could be explained on the basis of an intervening manuscript.

Let us look at still another passage where an insertion occurs. In *Gram.* 19, 17 Ott. includes in the text a quotation from Cicero, which was obviously a gloss on the word *gurgustio*.<sup>27</sup> At the corresponding point in Vat. we find this quotation in the upper left-hand margin in a hand that does not appear anywhere else in the three works under discussion.<sup>28</sup> It is conspicuously

<sup>27</sup> Si ut deus ipse melius habitaret antea videlicet tempore infinito in tenebris tamquam in gurgustio habitaret (*Nat. Deor.*, I, 9, 22).

<sup>28</sup> The formation of the letters is quite different from that of the original scribe: the & is made differently, *d*, *l*, *b*, *h*, have very high

labelled CICERO and has an insertion mark wrongly placed after *senectam* six lines below *gurgustio*, where it occurs in Ott. with no ascription to Cicero. It seems likely that the quotation appeared in the margin of the parent of our manuscripts with the reference mark so placed that Vat. overlooked it, while Ott. took it for an insertion mark and put the words into the text. A later hand in Vat. copied the quotation in the margin and indicated the wrong place for its inclusion.

The presence of the Cicero quotation could also be explained by supposing that an intervening scribe rather than the scribe of Ott. misread the reference mark. Another case where the evidence could be interpreted either way occurs in the *Indices* which precede the *De Grammaticis*. Here Vat. has two columns with *grammatici* in the first and *rhetores* in the second divided between folios 166 v and 167. Ott. has one and a half columns with the *rhetores* following the *grammatici* after a line of space, all on the same folio. It has, however, a displacement, which a comparison with Vat. may explain. Let us look at the end of the lists in each manuscript:

Vat. f. 167 (end of <i>grammatici</i> )	(end of <i>rhetores</i> )
C. Melissus	Iul. Tyro
M. Pomponius Marcellus	
Q. Reminus Palemon	
Val. Probus	
Ott.	(end of <i>rhetores</i> )
	C. Melissus
	Iul. Tyro
	M. Pomponius Marcellus
	Q. Reminus Palemon
	V. Probus

Here we assume that Vat.'s scribe copied his exemplar as he found it, but Ott. thought that the four *grammatici*, which were on the following folio, belonged at the end of the *rhetores* and that Tyro was meant to be inserted after Melissus. There is, of course, no proof that Ott. did not have before him an exact copy of Vat. instead of their common parent, but since either

shafts, while *s* and *q* go far below the line. It is the only marginal quotation of any kind in any of the three works. The fact that the ink is black and that a fine pen is not used makes me hesitate to assign this to the "corrector."

explanation is possible, in the light of other evidence we feel justified in the former interpretation.

If we are correct in our theory that Vat. and Ott. were both copied from the same lost descendant of σ, it is obvious that this ancestor retained many of α's abbreviations and that it was rife with marginal and interlinear variants. Years ago Rand pointed out that Vat.'s repetitions of *spectaculorum* (*Germ.* 24, 1) and of *de corpore contendant* (*Germ.* 24, 9) suggested in the first instance that the scribe began again a line which he had just finished, and, in the second case, that he went back two lines by mistake.<sup>29</sup> By counting the number of letters and spaces at these points in the printed edition which he was using, Rand concluded that the scribe was copying from a manuscript which had about 37 letters to the line "something to bear in mind if it should seem necessary to derive some other manuscripts directly from that archetype." This conclusion, however, is no longer tenable, since it now seems clear that these errors did not originate in Vat., but in its parent, so that the codex containing 37 letters to the line would be one step further removed. Examination of our two manuscripts at the point under discussion shows that although they agree in the former case in each having 35 letters and spaces, in the latter instance, where the printed edition showed 73 letters, Vat. has 64 and Ott. 70. If we apply Rand's method in attempting to explain the displacement of words in *Dial.* 9, 20, we find that Vat.'s line totals 58 letters and Ott.'s 60. From these discrepancies we can only conclude that it is unsafe to postulate the length of the line in the archetype by assuming that in every case where the scribe omitted or repeated words it indicates the omission of complete lines.<sup>30</sup>

That this latest manuscript of the *Germania* to make its appearance does not contribute new readings to the text is all too clear from the above report. That it throws no light on the vexed question as to how many manuscripts were brought to

<sup>29</sup> *A. J. P.*, XXVI (1905), p. 329, n. 1.

<sup>30</sup> Since Vat. has more abbreviations than Ott., it is probably a more accurate guide to the length of the line in their common parent. A folio selected at random (f. 179) shows that Vat.'s lines may vary from 49 to 54 letters. Even if we use the higher number 58 (as reported from *Dial.* 9, 20), the length of line in the archetype would have only 29 letters instead of 37 as conjectured by Rand.

Rome from Germany in the fifteenth century is equally obvious.<sup>31</sup> It does, however, to a certain extent, vindicate the scribe of Vat., who has long been blamed for errors, where it is now clear that he was merely "following copy." At the same time, it lends support to Robinson's belief from his study of the *Germania* that corruptions found in I (Vat.) had been passed down from  $\alpha$  to the  $\sigma$  group, where many of them were corrected in N and  $\Delta$ , but left "uncontaminated" in Vat. and Ott. Whether the parent of these codices was a brother of  $N\Delta$  or whether he was a brother of their parent, is a question that will require much further study of the manuscripts for all three works. An exhaustive study of the manuscripts of the *Dialogus* such as Robinson has made for the *Germania* will undoubtedly bring to light new facts concerning the relationship of the codices in the  $\sigma$  branch of the  $\alpha$  family.<sup>32</sup> The purpose of this paper has been merely to call attention to the existence of Ott. 1434, and to make some observations which grew out of the collation of this manuscript with Vat. Lat. 1518.

DOROTHY M. ROBATHAN.

WELLESLEY COLLEGE.

<sup>31</sup> Robinson, *Germ.* 1-14; C. W. Mendell, *A.J.P.*, LVI (1935), pp. 113-130.

<sup>32</sup> Professor Robinson's untimely death a few months ago put an end to his work on the *Dialogus*.

### "PISIDIAN." \*

Now, when professors of Sanskrit are turning their attention to Tibetan and Dravidian, it is regrettable that professors of Greek have almost turned their backs on the Anatolian neighbors of the Hellenes since the first brief flurry of activity following the discovery of the ruins of Troy, Mycenae, Tiryns, and Cnossus. For, if we may judge by the trade of primitive tribes, we may infer that commercial and intellectual intercourse between Asia and the Greek peninsula began long before the Homeric epics took shape, and that much of the history of ancient Greece may be revealed from discoveries in Asia Minor.

Scholars in Greek were among the first to investigate the non-Hellenic languages of Anatolia, and with considerable success—Saint-Martin and Schmidt on Lycian, W. M. Ramsay and W. M. Calder on Phrygian, for example. But recently such work as has been done on the "Anatolian" or "Asiatic" languages has been accomplished principally by scholars in other fields than Greek. And, while it may be well for the progress of our knowledge of the languages of Anatolia that non-Greek scholars occasionally approach the problems of the region from a divergent point of view, Greek scholars could more easily and competently carry the work to a thoroughly satisfactory solution.

For most of the languages of Asia Minor we have either no bilinguals or very short ones—for "Pisidian" none; for Phrygian none that are decipherable; for Lydian one short Lydian-Aramaic inscription; for Lycian not more than half a dozen, none parallel through the second sentence of epitaph formulas. I know nothing about Carian.

For the interpretation of these languages, scholars have resorted to Greek unilinguals, of which we have a great many scattered all over Asia Minor. And, if we progress in the interpretation of the ancient native languages of the country

\* The writer is glad to acknowledge his indebtedness to Professors Harold F. Cherniss, Joseph E. Fontenrose, and Edgar H. Sturtevant for advice and criticism. The writer is also indebted to Professor H. R. W. Smith for advice on archeology. I am alone responsible for any conclusions reached here.

beyond a few lines of text, I believe these Greek inscriptions afford us the most reliable guide when properly used.

Nearly all we know about Phrygian comes from Schmidt's discovery of a Phrygian Greek epitaph believed to parallel the imprecation formula of Phrygian. Calder stated:<sup>1</sup> "The attempt to interpret these texts must be founded mainly on a study of the Greek formulae used by the neighbors and friends of the dedicators. It is natural to suppose that the ideas expressed (according to the taste or education of the dedicato) in Greek or Phrygian, were roughly the same. It was by comparison with the prevailing Greek formulae against violation of the tomb that Schmidt, and especially Professor Ramsay, found their way to a true explanation of the common Phrygian formulae, and laid the basis for a scientific study of the language."

But—as in some interpretations of Lycian—the Greek unilinguals have not always been so well employed, but appear often to have been used to support the views of scholars who had already formed an idea of the interpretation of a certain word or passage. This was almost inevitable before Kalinka provided a corpus of part of the Lycian Greek inscriptions.

Arkwright, one of the keenest of the earlier interpreters of Lycian, remarked at the close of his final contribution to Lycian studies: "In the study of Lycian conjecture has too often been founded on conjecture. The structure has not been very lofty, but even so it has been more imposing than solid." Arkwright found that insufficient material made the process of internal comparison, carried on by Bacon's method of exclusion, rarely possible, and that interpretations by this method must therefore be considered conjectures. Instead, he proposed utilizing the Lycian Greek epitaphs, because "these later Greek epitaphs are a direct continuation of the older Lycian. When the Hellenization of the country began, the two languages were for a time used side by side, and the founders of the tombs expressed their wishes in the same terms, whichever they adopted." Arkwright was influenced by Oskar Treuber's analysis of Greek epitaphs into formulas, and if his resulting interpretations of Lycian are often incorrect, it is because he was still too much under the

<sup>1</sup> "Corpus Inscriptionum Neo-Phrygiarum," *J. H. S.*, XXI (1911), pp. 162-163.

influence of the conjectures of his predecessors to make a thorough revision of their interpretations.

In this study the point of view is that the etymological method is reliable when languages are closely related genetically and the phonetic equations are known, as for Avestic and Sanskrit, but that the method is not only useless but also misleading when the languages are not known to be genetically close; that the combinatory-morphological method seldom leads to anything but uncertain conjecture when the materials are too limited to test the interpretations in different contexts, as is true of the "Asianic" languages—as Arkwright observed for Lycian; but that the combinatory-morphological method combined with considerations of reality may progressively lead us to the correct interpretation of little-known languages. In an initial article on this, I pointed out that the commonly accepted interpretation of Lycian *hrppi* did not allow for the burial of the tomb owner and hence did not agree with reality. Whether interpretations of epitaphs are in accord with the type of tomb and its arrangements is, as we shall see later, another test of reality. And interpretations contrary to the similar inscriptions in other languages of the same area and as nearly of the same age as possible is another test of reality.

In this paper the primary concern is the interpretation of some texts in one of the non-classical languages of Asia Minor. The writer has neither the space, the inclination, nor the competence to analyze all the Greek inscriptions of Asia Minor. And, as most of the Greek monuments of that area have not been dated even roughly, it would be premature to undertake such a task at this time and at this distance from their location. I can only refer to some typical inscriptions from various localities and draw such deductions as possible from them. Yet a thorough analysis of the Greek inscriptions would not only bring rectifications in the interpretations proposed here, but would repay the efforts of the investigator in the light thrown on the life, customs, and nationality of the inhabitants. Some of the epitaph formulas were no doubt brought by the Greek colonists from their native cities, and the determination of this would fix the origin of the inhabitants. Others will reveal the influence of native customs on the Greek colonists, as for unlawful burial the provision of a penalty  $\Delta\dot{\imath}\Sigma\lambda\nu\mu\epsilon\iota$  on an epitaph inscribed at

Telmessus (Pisidia) by Αὐρ. Πηγορικὸς Βοιοτοῦ. Others would show the effects of Greek customs on the natives, one example of which will be noted in a later article.

#### A LANGUAGE OF PISIDIA.

In the early history of the Near East the chief importance of Pisidia, criss-crossed by mountain ranges, will probably turn out to be as a barrier to political unity and to the spread of culture; but the chief importance of the language or languages spoken there may, on the contrary, prove to be as a link or links between two or more genetically related languages spoken in surrounding areas—Phrygia, Lycaonia, Isauria, Pamphylia, Cilicia, Lycia, and Caria. A number of the languages of these countries have been grouped with Lydian under the designation “Anatolian,” and some scholars have thought they have found evidence of genetic relationship between one or more of them and Hittite and with the Indo-European family.<sup>2</sup> Hence the remains of a language spoken in Pisidia may be of some interest, not only to scholars working in the field of Asia Minor, but also to those engaged in Indo-European comparative grammar.

Except for Greek, Latin, and Phrygian, the only ancient language which has been found recorded in Pisidia was that engraved on some tombstones discovered by some villagers of Sofular, about two miles down the Eurymedon River from Imrohor (or Mirakhor), near the source of the river. These inscriptions were copied by William Mitchell Ramsay and David George Hogarth and published by the former under the title of “Inscriptions en langue pisidienne,”<sup>3</sup> basing his name for the language on a statement of Strabo’s. Since that time the language has been referred to as Pisidian.

Now if we had inscriptions in this language from all the principal cities of Pisidia—as we have of Lycian in Lycia,—there could be no reasonable doubt of the accuracy of the design-

<sup>2</sup> Most extensively by Holger Pedersen, *Lykisch und Hittitisch, Danske Videnskabernes Selskab, hist.-fil. Meddelelser*, XXX, No. 4 (1945); Piero Meriggi, “Der Indogermanismus des Lykischen,” *Germanen und Indogermanen, Festschrift für Hermann Hirt*, II (Heidelberg, 1936), pp. 257-282; and “Der indogermanische Charakter des Lydischen,” *ibid.*, pp. 283-290.

<sup>3</sup> *Revue des Universités du Midi*, I (1895), pp. 353-362.

nation "Pisidian." But the "Pisidian" inscriptions were found only near the northeastern border of that country, while Strabo only mentions Pisidian being spoken at a point beyond Pisidia's western frontier, as we shall see.

Probably the majority of educated men think of the Italian peninsula before the Roman conquest as populated by Romans or people very similar to them, such as the Oscans and Umbrians; but the specialist in that field knows of the Greek and Phoenician colonies, the Etruscans, Ligurians, Veneti, Gauls, etc. Similarly, we are finding that early Greece was far from homogeneous in population, for comparatively recent exploration and research have revealed the Minoan, Mycenean cultures, traces of the Leleges, etc. So it is hardly to be expected that Pisidia was populated entirely by Pisidian-speaking people. Therefore, before examining the inscriptions themselves, we may first consider the probable linguistic map of Pisidia, as nearly as the most ancient sources permit its reconstruction.

Neither Homer nor Herodotus mentioned Pisidia or the Pisidians. However, Homer spoke of Bellerophon and his son Isander fighting against the Solymi at the command of the king of Lycia,<sup>4</sup> which leads to the inference that they must have been neighbors of the Lycians and had probably been making raids on the richer Lycian valley lands.

Herodotus stated that in his time the land the Lycians occupied was formerly Milyan, and the Milyae were then called Solymi.<sup>5</sup> Strabo followed Herodotus in this, considering the better opinion was that Homer's Solymi were in his own time called Milyae.<sup>6</sup> We may note the territory which Strabo associated with the Solymi or Milyae:

οἵμαι δὲ καὶ τῶν Σολύμων τὰ ἄκρα τοῦ Ταύρου τὰ περὶ τὴν Λυκίαν ἔως Πισδίας κατεχόντων τὰ ίψηλότατα, . . . (I, 2, 10).

Σολύμους δ' εἶναι φασὶ τοὺς Καβαλλεῖς.<sup>7</sup> τῆς γοῦν Τερμησσέων<sup>8</sup> ἄκρας ὁ ὑπερκείμενος λόφος καλεῖται Σόλυμος, καὶ αὐτοὶ δὲ οἱ Τερμησσέων<sup>9</sup> Σόλυμοι καλοῦνται. πλησίον δ' ἐστὶ καὶ ὁ Βελλεροφόντου χάραξ καὶ

<sup>4</sup> *Il.*, VI, 184, 204.

<sup>5</sup> I, 173.

<sup>6</sup> XIV, 3, 10.

<sup>7</sup> MS x. Other MSS Καβαλλεῖς. See n. 10 below.

<sup>8</sup> Correction by Corais, for Τερμησσέως CDFmoxz, Τελμήσσεως rw, Τελμησσέων E.

<sup>9</sup> Instead of Τερμησσέως, CDFhx read Τελμησσέως, rw Τελμησσεῖς, Ei

ὅ Πεισάνδρου τάφος τοῦ νίοῦ, πεσόντος ἐν τῇ πρὸς Σολύμους μάχῃ.  
ταῦτα δὲ καὶ τοῖς ὑπὸ τοῦ ποιητοῦ λεγομένοις δμολογεῖται· περὶ μὲν  
γὰρ τοῦ Βελλεροφόντου φησὶν οὕτως·

δεύτερον αὖ Σολύμοισι μαχέσσατο κυδαλίμοισι·

περὶ δὲ τοῦ παιδὸς αὐτοῦ·

Πείσανδρον δέ οἱ νίὸν Ἀρης ἄτος πολέμοιο  
μαρνάμενον Σολύμοισι κατέκτανεν.

ἡ δὲ Τερμησσός ἔστι Πισιδικὴ πόλις ἡ μάλιστα καὶ ἔγγιστα ὑπερκειμένη  
τῆς Κιβύρας.

Λέγονται δὲ ἀπόγονοι Λυδῶν οἱ Κιβυρᾶται τῶν κατασχόντων τὴν  
Καβαλίδα, ὑστερον δὲ Πισιδῶν τῶν ὁμόρων οἰκισάντων . . . τέτταροι  
δὲ γλώτταις ἔχρωντο οἱ Κιβυρᾶται, τῇ Πισιδικῇ, τῇ Σολύμων, τῇ  
Ἐλληνίδι, τῇ Λυδῷ. . . . Μιλύν δ' ἔστιν ἡ ἀπὸ τῶν κατὰ Τερμησσὸν  
στενῶν καὶ τῆς εἰς τὸ ἐντὸς τοῦ Ταύρου ὑπερθέσεως δι' αὐτῶν ἐπὶ Ἰσινδα  
παρατείνοντα ὁρεινὴ μέχρι Σαγαλασσοῦ καὶ τῆς Ἀπαμέων χώρας (XIII,  
4, 16-17).

Some scholars have spoken of Cabalis as if it were composed of the four cities of the Cibyratae, but Strabo does not seem to use it so. As far as known to the writer the precise geographical location of Cabalis has not been determined, but it was evidently not far north of the Lycian border and on or beyond the western border of Pisidia, judging from the frequent association of Cabalis and Cibyra in Strabo, both at the beginning of the last passage quoted above and in the following:

Ὑπερβάλλοντι δὲ τὴν Μεσωγίδα τὴν μεταξὺ Καρῶν τε καὶ τῆς  
Νυσταῖδος, ἡ ἔστι χώρα κατὰ τὸ τοῦ Μαιάνδρου πέραν μέχρι τῆς Κιβυρά-  
τιδος καὶ τῆς Καβαλίδος, . . . (XIII, 4, 14).

Μετὰ δὲ τὴν Ἱεράπολιν τὰ πέραν τοῦ Μαιάνδρου, . . . τὰ δὲ πρὸς  
νότον ἡ Κίβυρα ἔστιν ἡ μεγάλη καὶ ἡ Σίνδα καὶ ἡ Καβαλὶς<sup>10</sup> μέχρι τοῦ  
Ταύρου καὶ τῆς Δυκίας (XIII, 4, 15).

**Τελμισσῆς.** See also n. 8 above. For the apparent confusion of *r* and *l*,  
compare Lycian *pilleñni*, ἐκ Πινάρων = Πιναρίδα in the bilingual (Ka-  
linka, 25, 6 and 12) where *-ll-* corresponds to Greek *-ναρ-*. The con-  
fusion in the writing of the name Termessus in Pisidia may be due to  
the greater familiarity of Greek copyists of Strabo with the Lycian city  
of Telmessus, a seaport on the west coast of Lycia; it may indicate that  
Strabo obtained his information from, or indirectly from, Lycian sources  
where apparent confusion of *r*, *l*, and *n* sometimes occurred; or it may  
indicate a confusion of *r* and *l* in both Lycia and Termessus, either  
because of genetic relationship of the languages spoken in the two  
places, to Solymi influence in both places, or to a phonetic peculiarity  
which may have crossed linguistic frontiers.

<sup>10</sup> MSS Dh read Καβαλλαῖς. See n. 7 above.

Thus Strabo seems to have believed that the Solymi occupied the whole of southern Pisidia from Termessus to Cabalis and Cibyra; and if the Milyae were the same as the Solymi, then their country extended as far north as Sagalassus and the country of the Apameians.

Modern archaeological exploratory work substantiates Solymi influence at Termessus, where epitaphs contain phrases such as<sup>11</sup>

. . . ἐπεὶ ὁ πειράσας ἐκτείσει Διὺς Σολυμεῖ \*<sub>a</sub>. (167, 6), . . . καὶ ἐκτείσει ἵερῷ τε Διὸς Σολυμέως \*<sub>d</sub> . . . (173), . . . ἐπεὶ ὁ πειράσας τι τούτων ἐκτείσει Διὺς Σολυμεῖ \*<sub>aφ'</sub>. (174), . . . ἱερέα Διὸς Σολυμέως . . . (39, 41), . . . ἱερέα θεᾶς Ρώμης Σεβαστῆς καὶ Διὸς Σολυμέως . . . (48).

The name of this god also appears on coins.<sup>12</sup>

That the language of the Solymi was still spoken at least within the memory of men living in Strabo's time, and that this language differed from Pisidian are indicated by the geographer's statement that the Cibyratae used the language of the Solymi and Pisidian.

Unfortunately Tetrapolis, the union of cities of the Cibyratae, is the only place where Strabo specifically mentioned the Pisidian language having been spoken, and this is west of the Pisidian border. Consequently Ramsay was not justified in jumping to the conclusion that his inscriptions were in the Pisidian language. Yet one may assume that the latter was probably spoken in most of the cities of Pisidia, even in territory once occupied by the Solymi.

So far Strabo has placed the Solymi in southern Pisidia west of Termessus, and the Milyae, which he believed were the same people, in central Pisidia. Strabo does not directly mention northwestern Pisidia. But he does indicate that a third language was spoken in Pisidia. In writing of the Leleges in the vicinity of Halicarnassus, he remarked:

. . . φασὶ δ' ἐν αὐτῇ καὶ ὅκτὼ πόλεις ψκίσθαι ὑπὸ τῶν Λελέγων πρότερον εὖανδρησάντων, ὥστε καὶ τῆς Καρίας κατασχεῖν τῆς μέχρι Μύνδου καὶ Βαργυλίων, καὶ τῆς Πισιδίας ἀποτεμέσθαι πολλήν (XIII, 1, 59).

<sup>11</sup> Numbers refer to inscriptions in *Städte Pamphyliens und Pisidiens* unter Mitwirkung von G. Niemann und E. Petersen herausgegeben von Karl Grafen Lanckoroński, II. Band, *Pisidien* (Vienna, 1892), pp. 196 ff.

<sup>12</sup> *Op. cit.*, p. 30.

And after enumerating from Artemidorus a list of the cities of Pisidia, ending with those of the Phrygian and Carian borders, Strabo added:

Τῶν δὲ οὖν ὁρειῶν, οὓς εἶπον, Πισιδῶν οἱ μὲν ἄλλοι κατὰ τυραννίδας μεμερισμένοι, καθάπερ οἱ Κίλικες, ληστρικῶς ἥσκηνται· φασὶ δὲ αὐτοῖς τῶν Λελέγων συγκαταμιχθῆναι τίνας τὸ παλαιόν, πλάνητας ἀνθρώπους, καὶ συμμεῖναι διὰ τὴν ὁμοιοτροπίαν αὐτόθι (XII, 7, 3).

Strabo gives his entire attention during the rest of the long paragraph to Selge, a Pisidian city. Logically this should follow the enumeration of the Pisidian cities, and it would seem that Strabo was distracted from his main theme here—the cities of Pisidia—by mention of cities on the Carian border, which led to thoughts of the Leleges. This seems the more likely when we consider that the portion of Caria occupied by the Leleges was on the extreme west coast; that this people would have crossed almost the whole of Caria to reach Pisidia and would not be likely to go far beyond the western frontier of that country; that, since Strabo gave considerable attention to the southwestern frontier but did not mention Leleges in that region, we may conclude that it was the northwestern part of Pisidia that the Leleges cut off for themselves, and that later they were driven back into the mountains and were being absorbed by the surrounding peoples. While it has never been determined who the Leleges were, we may infer that the Greeks did not consider them to be Carians, Pisidians, or any other peoples whom the Greeks recognized.

Linguistically Strabo left the northern and eastern portions of Pisidia blank. But I believe we have a clue to a fourth people, and perhaps a fourth language, in an apparent inconsistency of Strabo's accounts of the Homonadeis. Strabo did not specifically define the territory of the Homonadeis, but his references permit us to locate it roughly. He informs us regarding their homeland:

ἔστι δὲ ἐν ὑψηλοῖς τοῦ Ταύρου μέρεσι, κρημνοῖς ἀποτόμοις σφόδρα καὶ τὸ πλέον ἀβάτοις, ἐν μέσῳ κοιλον καὶ εὐγεων πεδίον, εἰς αὐλῶνας πλείους διηρημένον· τούτο δὲ γεωργοῦντες ὕκουν ἐν ταῖς ὑπερκειμέναις ὁφρύσιν ἡ σπηλαίοις, τὰ πολλὰ δὲ ἔνοπλοι ἥσαν καὶ κατέτρεχον τὴν ἀλλοτρίαν, ἔχοντες ὅρη τειχίζοντα τὴν χώραν αὐτῶν.

Συναφεῖς δὲ εἰσὶ τούτοις οἱ τε ἄλλοι Πισίδαι καὶ οἱ Σελγεῖς, οἵπερ εἰσὶν ἀξιολογώτατοι τῶν Πισιδῶν (XII, 6, 5-7, 1).

Such a country as Strabo described could hardly be found in Isauria,<sup>13</sup> about which he had been writing, but must be within the eastern frontier of Pisidia. The only region which would fit his description would be the valley of the Erymmedon above Selge and not far from the Pisidian-Isaurian frontier. The upper part of this valley had no known large cities, which is to be expected if the valley were inhabited by a wild tribe. We know that it was on the eastern border where the Homonadeis

<sup>13</sup> Despite Ramsay, who placed the homeland of the Homonadeis in Isauria, remarking "This identification of the plain of Lake Trogitis as the hollow surrounded by the Homanadeis will seem remarkable to the reader of Strabo who judges the geographer only from study at home" ("Studies in the Roman Province Galatia, I. The Homanadeis and the Homanadensis War," *J.R.S.*, VII [1917], p. 248). As probably not half a dozen educated persons now living have been in that region, and probably none of them had the Homonadeis in mind, Ramsay would seem to be placing his identification on an irrefutable basis. Yet eight pages before Ramsay had stated "Great part of the country of the Homonadeis lies very high. Their country included the main heights of the broad Taurus plateau, ranging between 5,000 and 7,000 feet above the sea, and broken by many ravines and canyons. . . . I have not crossed that part of the lofty Taurus ridge, . . ." Here he seems to be placing the Homonadeis where I have—along the Erymmedon River valley.

But later in fixing on the region around Lake Trogitis, Ramsay emphasized the importance of the term *αὐλῶν* in the passage from Strabo just cited above, stating that it "indicates a passage open at each end." This could well fit the long narrow valley extending from Lake Karalis to Lake Trogitis; but one may note that while lexicons give meanings denoting something long and narrow: "canal, trench, channel, strait, pipe," one may also find "*any hollow* between hills or banks, *a hollow way, defile, glen.*" If Ramsay had visited the main heights of the Taurus plateau, where he first placed the Homonadeis, could he have failed to find there the *αὐλῶνας* mentioned by Strabo?

Ramsay continued: "Strabo mentions the two lakes, Karalis and Trogitis, xii, 6, 1, and he gives no hint that he again is describing the valley of lake Trogitis in § 5 of the same chapter." And again: "his description of the Homanadensis country and the lake, which he does not name, depends upon the account given him by some officer who had taken part in the Homanadensis war. It is in every respect excellent, no word is wasted, every detail is full of meaning to those who have seen the country . . ." But could anyone who gave such a perfect description omit the chief geographical feature in that semiarid country—the lake?

Strabo started out chapter 6 (Bk. XII) with Lake Tatta, passes over

lived, because Strabo mentions the people subject to Antipater Derbetes (whose capital was in Isauria), and the Homonadeis, and several other peoples bordering on the Pisidians (XIV, 5, 24). In another passage, Amyntas captured Cremna in Pisidia and passed on into the country of the Homonadeis (XII, 6, 5), from which we may infer that the Homonadeis territory extended close to Cremna.

Who were the Homonadeis? Strabo does not tell us directly but I believe he gave us hints of their nationality in his account of Amyntas, who had built a royal residence at Old Isaura:

ἐν δὲ τῷ αὐτῷ χωρίῳ καὶ νὸν τεῖχος οἰκοδομῶν οὐκ ἔφθη συντελέσας, ἀλλὰ διέφθειραν αὐτὸν οἱ Κίλικες, ἐμβάλλοντα εἰς τὸν Ὄμοναδεῖς καὶ ἐξ ἐνέδρας ληφθέντα.

. . . ἐπειράτῳ τὸν ἐκ τοῦ Ταύρου κατατρέχοντας Κίλικας καὶ Πισίδας τὴν χώραν ταύτην, Φρυγῶν οὖσαν καὶ Κιλίκων, ἐξαιρεῖν, . . .

. . . ὁ δὲ οὖν Ἀμύντας τὴν μὲν Κρῆμναν εἶλεν, εἰς δὲ τὸν Ὄμοναδέας παρελθών, οἱ ἐνομίζοντο ἀληπτότατοι, καὶ καταστὰς ἡδη κύριος τῶν πλείστων χωρίων, ἀνελὼν καὶ τὸν τυράννον αὐτῶν ἐξ ἀπάτης ἐλήφθη διὰ τῆς τοῦ τυράννου γυναικός. καὶ τοῦτον μὲν ἐκεῖνοι διέφθειραν, . . . (XII, 6, 3-5).

We may note several apparent inconsistencies in this account. In the first paragraph of the citation, while Amyntas was invading the country of the Homonadeis, the Cilicians killed him. What were the Cilicians doing in the country of the Homonadeis? Strabo never says.

But in the second paragraph, Strabo tells us that Amyntas was trying to exterminate the Cilicians and Pisidians. Amyntas captured Cremna, which we can understand as part of his campaign against the Pisidians; but if he were trying to exterminate the Cilicians why did he then invade the country of the Homonadeis?

the plains of Lycaonia where Amyntas had 300 flocks, mentions Lakes Caralis and Trogitis, Iconium, the Cappadocian border and Cilician border of Lycaonia-Isauria. He does not say so, but he is evidently describing the Lycaonian-Isaurian possessions of Amyntas—presumably firmly in his hands—and leading logically to mention of the Phrygian and Pisidian lands he claimed, and the war against the Homonadeis. Trogitis, then, was already secured. And one may also note that Amyntas did not start out from Old Isaura, his royal residence, very close to Trogitis, to subdue the Homonadeis, but from Cremna in east central Pisidia.

And, while in the first paragraph Cilicians killed Amyntas, in the third "those (people)," the Homonadeis, killed him.

Strabo would seem to have written this account in a wine shop when ideas had become confused. But there is no confusion when we realize that the Homonadeis or their tyrants must have been Cilicians, a fact which Strabo either did not know or did not think necessary to explain.

In the second paragraph, where the Pisidians and Cilicians overrun the country of the Phrygians and Cilicians, Κιλίκων, has been interpreted as a slip for Δυκαόνων. For a writer as imbued with Roman imperialist views as Strabo, a slip it probably was from a legalistic point of view, for Amyntas appears to have held the greater part of his territories under the Romans; but if the Homonadeis were Cilicians, it would be a *de facto* statement to say that the Pisidians and Cilicians (i. e., Cilician Homonadeis) were overrunning τὴν χώραν ταύτην, Φρυγῶν οὖσαν καὶ Κιλίκων, both claimed by Amyntas. We may surmise that the situation was this:

The Phrygian-Pisidian border area was ill-defined and both countries claimed it. Strabo informs us that Amyntas had possession of the country from Antiocheia to Apameia Cibotus. The Pisidians were, then, making forays into the northern borderlands which they also claimed. And on the eastern border of Pisidia were also lands claimed by Amyntas: Lycaonia, Isauria, and Cilicia Tracheia. And on this borderland were the Cilician Homonadeis, wild and warlike καὶ κατέτρεχον τὴν ἀλλοτρίαν. They were no doubt making trouble for Amyntas in Lycaonia and Isauria. Thus the Homonadeis Cilicians would be overrunning the country of the Cilicians—their own country by right of occupation, but illegally from the point of view of Amyntas.

This solution—that the Homonadeis were Cilicians—is the more probable when we consider that the northwest corner of Cilicia extends to the southeast corner of Pisidia, and the Homonadeis as a northward extension of Cilicians into Pisidia would not be unlikely, particularly as Cilicia included Isauria in Alexander's time, and hence probably in Persian time.<sup>14</sup>

<sup>14</sup> Ramsay, *op. cit.* (in n. 13 above), p. 251, reached the conclusion that the Homonadeis were Cilicians. But Horace Leonard Jones, in the latest edition of the *Geography* that I have seen, either was unaware

Thus Strabo leads us to infer that the language of the Solymi was spoken in the southwest of Pisidia and perhaps in central Pisidia, the language of the Leleges in the northwest, Pisidian in most of the chief cities, and Cilician near the eastern frontier.

Nothing in Strabo's *Geography* indicates that he had detailed information about the northern part of Pisidia. Johannes Friedrich published Phrygian inscriptions from "Borlu zwischen Apollonia und Antiochia in Pisidien," "Neapolis in Pisidien," and "Apollonia in Pisidien," but these were not reckoned as Pisidian cities in Strabo's list taken from Artemidorus.

As the inscriptions recorded by Ramsay and Hogarth came from the northern part of Pisidia, on which Strabo was least well informed, we are not warranted in calling this language Pisidian in a definitive manner. Cilician would seem more likely, but as we do not know how far north the Homonadeis extended, we cannot definitely assign the inscriptions to that language. We can only say that they may be Cilician. The sculpture engraved on Ramsay's steles was rude such as we might expect from a people not highly civilized. At present all we can say is negative—the language of the steles is not Phrygian. And if the language is referred to hereafter as "Pisidian," it will be understood to refer to the language of Ramsay and Hogarth's steles.

#### OTHER INSCRIPTIONS OF PISIDIA AND BORDER AREAS.

Of course the best beginning we could make on "Pisidian" would be by means of a bilingual, but we have none. As far as known to the writer only two attempts have been made to interpret the inscriptions found by Ramsay and Hogarth. The first of these, by Ramsay,<sup>15</sup> appears to have been based on his knowledge of Greek epitaphs, but suffers from his application of one feature of these,—personal names,—to the exclusion of others, to the interpretation of the "Pisidian" steles. The second attempt, by Wilhelm Brandenstein,<sup>16</sup> is an admirable example of

of Ramsay's interpretation or, justifiably, disregarded Ramsay's almost categorical statement as not borne out in the passages of Strabo that Ramsay cited.

<sup>15</sup> *Op. cit.* (in n. 3 above).

<sup>16</sup> "Die Sprache der Pisidier," *Archiv für Orientforschung*, IX (1934), pp. 52-54.

the combinatory-morphological analysis. His interpretations make sense and generally are morphologically consistent. Yet, as pointed out above, however much of an advance the combinatory-morphological approach may be to the etymological method, it is still insufficient if contrary to reality. And linguists are too apt to regard a morphologically correct and semantically satisfactory sentence as expressing fact.

Brandenstein's fault, like most of the interpreters of "Anatolian" epitaphs—except Phrygian—that I have seen, was that he did not know, or disregarded, what the tomb builder was likely to put down in his epitaph. He might have gained this knowledge by a survey of the epitaphs of the region.

We may illustrate this from the nearly parallel situation in Lycian studies where we have only half a dozen Greek-Lycian bilinguals, most of them epitaphs, sometimes broken or nearly obliterated, and never even fairly parallel through the second sentence. As a result of this, Lycian Greek unilinguals have been utilized almost from the beginning of Lycian studies as an aid in interpretation. But, except for Arkwright, I know of no scholar who has used the analysis of the formulas of a number of Greek inscriptions to lead him to an original interpretation of a Lycian word or passage.

As inscriptions in Greek and in native languages of Anatolia are often separated in time by some centuries, there is no *a priori* reason why they should be parallel, and we may guess that the Lycian scholar resorted to Greek unilinguals partly because he knew Greek, partly because he found a Greek epitaph which supported his view, and, subconsciously, because of the prestige which anything Greek held in the learned world. Yet we may find more cogent reasons for using unilinguals of known meaning for interpreting an unknown, or little known, language. Let us take as examples inscriptions in Lycian, of which we have more than from any other "Anatolian" language, and in New Phrygian, which is better known to scholars.

1. Lycian Greek and Lycian inscriptions sometimes are contemporary and sometimes the former are several centuries later than the latter. With the lapse of time, customs and ideas no doubt changed and we cannot expect the Greek and Lycian epitaphs to be entirely parallel. Yet one may note that the

barefooted, mantle-wearing, chariot-riding Lycians were some 1,500 years closer in civilization and thought to the Greeks of the Roman Empire than any scholar can be today when he expresses on a typewriter what he conceives the Lycians must have put into their epitaphs.

2. Both the Greeks and Lycians were mainly concerned about three things when they wrote their epitaphs: stating that they had built the magnificent tomb; getting themselves and their immediate relatives buried in it; and keeping out everyone else. Because of these necessities we may expect a good deal of parallelism in essentials.

3. Influence of a superior culture may be dominant, but it is probably never an entirely one-sided affair. Thus, although Greek had superseded New Phrygian in the first part of epitaphs of the region, the native language had influenced not only the pronunciation of Greek but also the syntax and formulas, as Ramsay long ago pointed out.<sup>17</sup> We shall find evidence of a similar sort of interaction of Lycian and Lycian Greek. Consequently one may expect considerable correspondence owing to the interaction of the two civilizations and languages.

4. Religious influence is a powerful influence for conservatism. For, while curses as a deterrent to tomb robbers had generally been abandoned in Phrygian Greek inscriptions in favor of payment to a treasury, Phrygian imprecations still prevailed in many inscriptions<sup>18</sup> and even in one inscription entirely in Greek. Consequently we can expect to find parallelism in some inscriptions because of religious influence.

We may make the following observations from a survey of a large number of epitaph formulas of Pisidia and surrounding areas, of which representative specimens will be noted below:

<sup>17</sup> Ramsay, "Phrygian Inscriptions of the Roman Period," *K. Z.*, XXVIII (1887), p. 386, noted that the execratory formula of no. 1 was "evidently composed by a person to whom Greek was a strange language"; *πος* for *προσ*, *ητω* instead of *εστω* influenced by Ph. *ειτον*; and p. 398, Phrygian dative transferred to Greek.

<sup>18</sup> "Two reasons are conceivable why a curse against the violator of the tomb should be expressed in Phrygian, while the rest of the epitaph is in Greek: either the former was thought more religious and more likely to be efficacious with the gods of Phrygia, or it was more intelligible to the mass of the people" (Ramsay, "Phrygian Inscriptions of the Roman Period," *K. Z.*, XXVIII [1887], p. 382).

The content of an epitaph depends largely upon its length. For the tomb builder the order of importance was

1. *Name* of the person or persons to be buried. (Generally a single grave.)
2. Name of the tomb builder; person or persons to be buried, usually *identified by relationship to tomb builder*, less frequently by name and then generally only additionally.
3. Name of the tomb builder; a reference to the tomb (either a noun expressing a type of tomb, a verb referring to the building of the tomb, or both); person or persons to be buried (identification as in no. 2 above).
4. Name of the tomb builder; reference to the tomb; person or persons to be buried; penalty for violation of the tomb.

The only variation in these essentials known to the writer is in New Phrygian, where, if the inscription is short, the Phrygian imprecation alone may have been used.<sup>19</sup>

<sup>19</sup> This is very doubtful. A check of the Phrygian inscriptions in *Monumenta Asiae Minoris Antiqua*, Vols. I (W. M. Calder), IV (W. H. Buckler, W. M. Calder, W. K. C. Guthrie), VI (Buckler, Calder), reveals that of those inscriptions without the usual Greek dedication the authors mention several that are incomplete—either broken or the dedication was on another stone now missing. Another monument, no. 385, Vol. I, a doorstone, with a Phrygian imprecation on the molding above the door, seems to be complete but has no Greek dedication; but Calder noted that the lower panels of the door "were empty or have been defaced." The improbability of any Phrygian imprecation inscription having been the complete inscription is most apparent in Vol. I, where Calder gave a more complete description of the monuments than is found in later volumes.

Yet sometimes a Phrygian imprecation may have been all there ever was of some inscriptions; but this was probably not the original intent of the grave owner. Calder believed that Phrygian continued to be spoken in the most backward communities. There the owner may have had to wait until someone passed through who could write out the message in Greek and someone else who could write the Phrygian portion, and then he had to have one or the other inscribed as he could find a stone cutter who was able to do the work. Thus the owner of the white marble pediment (no. 17, Vol. IV) probably had the Phrygian curse inscribed first because he found someone who could write it out or inscribe it before he found someone who could write or inscribe the Greek dedication below. But sometimes the owner may have died or left the country before he could get the Greek dedication inscribed, and so we find a Phrygian imprecation without a dedication. Inscriptions

As the Pisidian epitaphs are too short possibly to contain a penalty formula, the Greek epitaphs of type 4 above, containing a penalty beginning with ἀλλω δὲ οὐδενὶ, ἐτέρῳ δὲ μηδενὶ, etc.,<sup>20</sup> have generally been eliminated from consideration at this time and will be taken up in a later article on Lycian; the few examples of type 4 referred to below may be taken as further substantiating type 3 formulas. Type 1 formulas, containing only the name of the person to be buried or his further identification, will be examined later in this article. Epitaphs of types 2 and 3—containing the name of the builder; (reference to the tomb); person or persons for whom the tomb was built—usually designated primarily by relationship to the tomb builder—are the prevailing types all around the spot where the "Pisidian" inscriptions were found,<sup>21</sup> at Sagalassus,<sup>22</sup> Hissardshik (Selge?),<sup>23</sup> Termessus,<sup>24</sup> Apameia (east of location of the "Pisidian" inscriptions),<sup>25</sup> Apollonia (northeast of location of the "Pisidian" inscriptions),<sup>26</sup> Ada Köi (south of Kirili or Shehir Göl [Caral-litis Lacus, Caralis Palus] in Isauria-Lycaonia, closest to location of the "Pisidian" inscriptions),<sup>27</sup> Ortabadem Han (east

where the owner had the Greek dedication inscribed but no Phrygian imprecation we put down as complete because most Greek epitaphs lack imprecations. So I am inclined to believe that an imprecation formula alone may never have been the intent of the grave owner during the period when New Phrygian inscriptions were set down.

<sup>20</sup> These words are not strictly the beginning of the penalty formula but of the prohibitive formula which precedes it in many Greek epitaphs. This prohibition does not occur in Phrygian but the main part of its content is incorporated in the penalty formula, and the same is true of Lycian, as I shall note elsewhere.

<sup>21</sup> Numbers of Pisidian Greek inscriptions refer to Niemann and Petersen, *op. cit.* (in n. 11 above); of Greek inscriptions from Lycaonia, Isauria, and Pamphylia, to Heinrich Swoboda, Josef Keil, and Fritz Knoll, *Denkmäler aus Lykaonien, Pamphylien und Isaurien* (Brünn, Prag, etc., 1935); Lycian Greek and Lycian to Ernst Kalinka, *Tituli Asiae Minoris* (Vienna, 1901, 1920); and Phrygian Greek to *Monumenta Asiae Minoris Antiqua*, Vols. IV and VI.

<sup>22</sup> Type 2, nos. 231, 226, 225, 223; type 3, no. 232.

<sup>23</sup> Type 3, 260.

<sup>24</sup> Type 2, 110, 102, 176, 177; type 3, 168, 153, 172; type 4, 169, 167, 174, 165, 149.

<sup>25</sup> Type 4, 187, 208, 213.

<sup>26</sup> Type 2, 174; Type 3, 173, 178.

<sup>27</sup> Type 3, 99 (left and right), 100.

of Gembös Göl),<sup>28</sup> Geldshik Ören (Etenna?, in the mountains NE of Side, Pamphylia),<sup>29</sup> Tlos (Lycia).<sup>30</sup> We may also note the following two Lycian inscriptions from Limyra:

Type 3. No. 123. *ebēnnē kupā mē ti prīnawatē eketeiya hrppi atli ehbi se ladi ehbi se tideime.*  
This grave built Eketeiya for-himself  
and wife his and children.

No. 117. *ebeiya erawaziya me ti prīnawatē sideriya p[ar]m[ēnah Tò μημα τόδ' ἐποιήσατο Σιδάριος Παρμένοντος tideimi [h]rppi etli e[h]bi se ladi ehbi se tideimi pubieleye.*<sup>31</sup>  
vīls éavntāi καὶ τῇ γν[να]ικὶ καὶ vīlāi Πυβιάλη[ι].

The Lycian epitaphs just cited show the word order:

1. Reference to the tomb.
2. Name of the builder.
3. Person or persons for whom the tomb was built.

A great many of the Lycian Greek epitaphs follow the same order, perhaps because of Lycian influence. For nearly all other Greek epitaphs and even many Lycian Greek epitaphs (and some Lycian epitaphs—through Greek influence?—) follow the order:

1. Name of the tomb builder.
2. Reference to the tomb.
3. Person or persons for whom the tomb was built.

When one of the steps is omitted, it is the second; the relative order remains the same.

#### "PISIDIAN" INSCRIPTIONS.

A survey of neighboring inscriptions, then, leads us to expect that the three steps will also be found in "Pisidian." And both the Lycian order of these steps and that of most other epitaphs agree in making the final step refer to the persons to be buried.

<sup>28</sup> Type 3, 104.

<sup>29</sup> Type 3, 110.

<sup>30</sup> Tlos was selected from Lycian cities because we have extensive Greek inscriptions there and it is one of the closest Lycian cities to Pisidia. Type 2, 639, 638, 596, 597 b; type 3, 597 a; type 4, 627, 602, 603 a, 618.

<sup>31</sup> Lycian Greek nos. 602, 603 a, and Lycian 123, 117 (cited above) are more representative of the usual word order in Lycia.

But Brandenstein made the last step of "Pisidian" epitaphs refer to the tomb owner, contrary to practically all epitaphs of the region. Yet this does not prove Brandenstein wrong, but merely indicates a probability that he was.

For the interpretation of "Pisidian" the internal evidence of the inscriptions themselves—the uniformity or diversity of the words falling in the different steps of an epitaph—provides our most reliable guide. For the name of the tomb builder we should expect great diversity. For a reference to the tomb we should expect very few different words. For the persons to be buried we should expect two words to occur rather often—those for "self" and "wife,"—together with some others denoting other relatives and a few personal names, i. e., moderate diversity. We may now note all "Pisidian" inscriptions containing more than two words.

1	2	3
4. μεμονα	μουσητρα	μας ιαος
5.	μουσητρα	τας
7.	μουσητρα	γδασας
3.	μο σητρα	δ[ο]τες
2. λιρ	μουσητρος	δοτες
6. γπουρωξα	μουσητρος	
1. δωταρι	μο σητως	δωτ[α]ρις δωταρι ενεις
9.		δοταις
10. μενονα	γδαβος	ονανις [β]αβ[ον]
11. γα	γδαβος,	
εδα	γδαβος	
13. μηνει	γδεβε	τις ενα ουπερ
14. εια	γδεβε	δοταις
12. γδαβα μηνει	γδεβε	τις

The most striking thing about this list is column 2, where it is obvious that only two roots occur. Anyone familiar with the constant recurrence of *κατεσκειαστεν* and *σωματοθήκη*, *μηημειον*, or *ἡρώων* in Lycian Greek epitaphs, of *prñinawatē* and *kupā* or *prñinawā* in Lycian epitaphs, will infer that the two recurring "Pisidian" roots must represent a reference to the tomb, the second step in most of the epitaph formulas we have just surveyed. Brandenstein and I are in general agreement on this point.

Contrary to the impression given by column 2, the most strik-

ing thing about column 1 is its diversity. The *μῆνες* in the last epitaph (no. 12) is probably the same stem as the *μῆνει* in no. 13, but it could not be placed directly under the other words of column 1 without destroying the symmetry of column 2. But otherwise all the words of column 1 are different. This is what we should expect of the names of tomb owners.

Column 3 does not have the very limited number of roots found in column 2 or the diversity of column 1. *δοτ-* occurs four times, *τι-* thrice, with several other stems occurring probably only once each. Anyone familiar with the *ἴαντω καὶ γυναικὶ αὐτοῦ καὶ τοῖς τέκνοις αὐτῶν*, or the Lycian *hrppi ladi se tideime* "for himself, (his) wife, and children," will realize that *δοτ-* and *τι-* must represent "self" or "wife," and the other words in column 3 either relationship terms or names of persons, probably generally the former.

Thus the degree of diversity of columns 1, 2, and 3 leads to the same conclusion regarding their content that one would deduce from the majority of non-“Pisidian” epitaphs that one finds in Pisidia and surrounding countries.

It is on the content of columns 1 and 3 that my interpretation of the “Pisidian” inscriptions differs radically from that of Brandenstein. We both agree that column 1 contains personal names,<sup>32</sup> but I conclude that they can only be the name of the tomb builder and in the nominative, while Brandenstein believed them to be the names of the persons to be buried and in the dative, contrary to the custom in other epitaphs of Pisidia and vicinity. I conclude that column 3 contains references to the beneficiaries of the tomb builder, frequently referred to by their relationship to him, and in the dative case, while Brandenstein thought they were the names of the tomb builders in the nominative case—contrary to the custom in the other epitaphs of Pisidia and vicinity; he considered *δοτ-* to be a verb and *τι-* to be part of a verb, and translated only one word as a relationship term—which again is contrary to the general custom of the region. All of the above conclusions of Brandenstein run against the criterion of diversity.

<sup>32</sup> My conclusion was reached solely on the grounds of the diversity of the words of column 1, which would be expected of personal names. Brandenstein based his conclusion on similar personal names in Greek

Brandenstein was probably led astray in part by the nominative in -s found so frequently in Latin and Greek, in part by the frequent nominative in -s found in masculine personal names in the Greek inscriptions of Pisidia. As for the latter, we may note that Lycian masculine names ending in a vowel, usually -a, in the nominative were frequently hellenized by the addition of -s in the Greek inscriptions of Lycia, so that we are not warranted from the appearance of -s in Pisidian Greek nominatives of masculine nouns in inferring that "Pisidian" also had -s in the nominative. As to the first consideration mentioned above, Lycian nominatives end more often in -a than any other vowel and six of the nine "Pisidian" nouns that I consider to be nominatives end in -a; and in the present state of our knowledge "Pisidian" is at least as likely to be related to Lycian as to Greek or Latin.

The chief considerations for my interpretation of the content of columns 1-3 have now been stated. I have been guided in my interpretation of column 3 by the human figures on the steles. This is not necessarily a reliable criterion, as the male erector of a monument to his wife alone might conceivably have engraved a family scene in memory of happier days; yet it happens that the figures on these "Pisidian" steles can be interpreted consistently, although the reader will realize that the interpretations must remain hypothetical unless skeletal remains can be found which will substantiate them. My interpretations follow:<sup>33</sup>

1	2	3	Figures
4. μεμονα Memoua	μονοσητα stele	μας ιαος for family his	1 m, 1 w, baby
5.	μονοσητα stele	τας for Ta	1 youth
7.	μονοσητα stele	γδασας for son (for Gdasa?)	1 youth

inscriptions found in Pisidia or neighboring territory (*op. cit.*, pp. 53-54).

<sup>33</sup> The figures engraved on the steles are listed on the right: m, man; w, woman.

1	2	3	Figures
3.	<i>μοσητα</i> stele	<i>δ[ο]τες</i> for husband	1 man with pike
2. <i>λιρ</i> Lir	<i>μονσητος</i> made	<i>δοτες</i> for wife	1 w
6. <i>γπουρωξα</i> Gpourōksa	<i>μονσητος</i> made		1 m
1. <i>δωταρι</i> Dōtari	<i>μοσητως</i> <i>ειη</i> made              this (?)	<i>δωτ[α]ρις</i> for Dōtari (and) <i>δωταρι</i> <i>ενεις</i> <sup>34</sup> of Dōtari for children	2 m, 1 w
9.		<i>δοταις</i>	
10. <i>μενοua</i> Menoua	<i>γδαβος</i> erected	For Eiani, wife (D.)	1 w
		<i>ονανις</i> [β]αβ[ον] for Ouani ?	1 m, 1 w
11. <i>γα</i> Ga	<i>γδαβος</i> erected.		
<i>εδα</i>	<i>γδαβος</i>		
Eda	erected.		1 m, 1 w
13. <i>μηνει</i> Mēnei	<i>γδεβε τις</i> erected	<i>ενα ουπερ</i> <i>δοταις</i> and also              for wife	1 m, 1 w
14. <i>εια</i> Eia	<i>γδεβε</i> erected	<i>τις</i> for self	1 w
12. <i>γδαβα μηνει</i>	<i>γδεβε τιε</i>		
Monument (A.) for Mēnei erected he-himself			1 m

We may take up the epitaphs one by one and see that the interpretations offered here often present additional indications that we are on the right track:

4. Brandenstein: "Dem Memouas zum Andenken. Mas (und) Iaos." Brandenstein must break up the epitaph into two disjointed sentences meaningless by themselves. This type of epitaph does occur in the region (see Termessus, no. 177), but it is rare. And a final double signature is even rarer and therefore more improbable.

My interpretation of *μας ιαος* is based on the figures of a man, a woman, and a baby on the stele. "Family" seemed the only meaning which would cover them all. I had no conscious reason for interpreting *μας* as "family" and *ιαος* as "his," rather than

<sup>34</sup> Cf. Gr. *Ivis*.

the reverse, but may have been influenced by Greek γυναικὶ αὐτοῦ and Lycian *ladi ehbi* "for wife his," etc. One may suggest that if my interpretation is correct, *ιαος* (3d pers. pron., D.) may be connected with Phrygian *ιος* (indefinite pron., N.), although the inflection of the two languages is not the same.

I have followed Brandenstein in considering *μουσητα* a noun, not in the dative as Brandenstein thought, but in the accusative. This would make this accusative, in -*a*, the same as most of the nominatives in column 1, also in -*a*. At first the Indo-Europeanist, accustomed to much more complex declensions, may incline to skepticism of such an inference, and certainly the equation deserves further consideration before a definite conclusion can be drawn. But one may point out that if there are any accusatives in the above epitaphs they could not have ended in -*m* or -*n* as the Indo-Europeanist might expect, for nasal final consonants are not found; that in Lycian, one of the languages geographically closest to "Pisidian," the most common ending of the accusative is a phoneme which has been transcribed—with some justification—as -*ã*, which has been correlated by some scholars with the Greek accusative singular -*v*, IE \*-*m*;<sup>35</sup> that the New Phrygian -*v* in the accusative disappears in some inscriptions; that along an axis from Phrygia to Lycia one may infer that a final nasal consonant of the accusative had probably nasalized the preceding vowel, then dropped, and that in the "Pisidian" territory the Greek language was inadequate to record a nasalized vowel, or the degeneration had progressed a stage further there and the nasalization had been lost.

In no. 12, I do not see any way of translating the epitaph except by considering *γδαβα* as an accusative; we shall return to this in the proper place below. But this is the only word where an accusative seems necessary. In considering *μουσητα* as a noun, I have followed Brandenstein, trying to retain the best of his interpretations where this seemed possible. To Brandenstein's translation of *μουσητα* the Greek inscriptions of Termessus lend some support. After the name of the tomb builder occur *τὴν σωματοθήκην ἔαντω . . .* (156, 158, 167, 169), *τὴν θήκην ἔαντη μόνη . . .* (147), *τὴν σωματωθήκην ἔαντοις . . .* (162), *τὴν σωματοθήκην ἔαντη* (168), *τὴν σωματοθήκην Διωτείμω . . .* (153). This

<sup>35</sup> By Holger Pedersen, *op. cit.* (in n. 2 above), p. 15, with Hit. -*n*.

occurs in the usual formula at Termessus, and seldom do we find, as in no. 165, *κατέστησεν σωματοθήκην ἔαντῃ*. . . . As the Termessian inscriptions far exceed in number all the others together from Pisidia, they would be likely to lead one to conclude that "Pisidian" *μουσητ-* and *γδαβ-* were nouns, as Brandenstein concluded for most of their occurrences. But Termessus was on the opposite side of Pisidia—in the south—from where Ramsay's steles were found, and in territory that Strabo considered to belong, or to have belonged, to the Solymi. Whether the language of the Solymi influenced the wording of the Greek epitaphs at Termessus is something we cannot answer.

We may note, however, that the longer epitaphs of Pisidia and vicinity in non—"Pisidian" languages, while agreeing in having an opening formula of types 2 or 3 above, did show a great deal of regional variation in detail. Thus, although the epitaphs at Termessus generally employed a noun to refer to the tomb, those of Sagalassus employed neither verb nor noun to refer to the tomb except in one epitaph where both were employed. At Hisardshik (Selge?) the one epitaph employs a verb only. At Ada Köi a verb alone is used twice, a verb and noun once. In Lycian inscriptions both verb and noun are nearly always employed. These regional peculiarities extend to individual words used in the Greek epitaphs: *ἐποίησεν* at Apameia, *ἀνέστησεν* at Ada Köi, *κατέστησεν* at Termessus, *κατεσκεύασεν* generally in Lycia, but *ἐωνήσατο* frequently at Xanthus. Some of these peculiarities were probably brought from their homeland by Greek immigrants and may provide valuable clues to their origin; others probably developed locally among Greek colonists; but some are probably the result of native influence.

But here the point in which we are interested is that the inscriptions nearest the area of the "Pisidian" inscriptions employ a verb and not a noun as Brandenstein's interpretations imply for the "Pisidian" inscriptions.

Consequently, I am personally inclined to think that *μουσητ-* and *γδαβ-* are generally verbs, that *μουσητος* and *γδαβος* represent the same verbal flection, and *μουσητα* and *γδεβε* another verbal flection, the two verbs being of different "conjugations."

Yet *γδεβε* in its occurrences is always associated with *τι-* "self" or "himself," so that *γδεβε* may be a sort of reflexive verb form. And as *γδαβα* must be regarded as a noun in the

accusative, we might infer that *μονσητα* also is. In the present state of uncertainty whether *μονσητα* is a verb or noun, I have translated it as a noun in the interpretations, considering it to be a verbal noun formed from *μονσητος*.

As *κατασκευάζω* is probably the most common verb in the introductory formulas of the Greek inscriptions in the region under discussion and the corresponding verb in Lycian is *prñnawat-*, with the verbal noun *prñnawa*, I have translated *μονσητος* "made," and *μονσητα* "something made, stele." "Monument, memorial," a meaning more in accord with Brandenstein's interpretation, seems less likely; for, while *μνήμης χάριν* occurs frequently in the region, it is usually at the end of the epitaph, which is contrary to the general use of *μονσητα*; moreover, *μνήμης χάριν* usually occurs on single graves, while *μονσητα* is used on family graves as well.

5. Brandenstein: "Zum Andenken (hat es aufgestellt) Tas." Here again Brandenstein must supply something to make sense. As nos. 5 and 7 have figures of a youth engraved on the steles, we might interpret either *tas* in no. 5 or *γδασας* in no. 7 to mean "for son," but as both are not likely to have that significance, I have tentatively identified *γδασας* with that meaning; for we find *tas* occurring as a personal name in the region east of Carallitis Lacus and west of Laodiceia and Iconium,<sup>36</sup> i. e., the territory east of the locality where the "Pisidian" texts were found. This does not necessarily imply any genetic relationship between "Pisidian" and the language spoken east of the lake any more than my own name of Shafer at the end of this article warrants the conclusion that a sort of corrupt German must have been the language once spoken at Berkeley, California.

7. Brandenstein: "Zum Andenken (hat es aufgestellt) Gdasas." See comment on no. 5 above.

3. Brandenstein: "Zum Andenken wurde (es) aufgestellt." Brandenstein's interpretation lets the tomb builder say only what was obvious to the spectator. If we follow Brandenstein, the tomb owners did not say much. My own opinion is that they would make good headline writers.

<sup>36</sup> Once at Eiret (no. 4, *Denkmäler*—see n. 21 above), once at Kulu Kissa, and frequently at Kadyn Khan (W. M. Calder, *Monumenta Asiae Minoris Antiqua*, Vol. I [Manchester University Press, 1928], index).

There is some doubt about the interpretation of this inscription. The meaning "wife" is appropriate for *δοτ-* in its other occurrences, nos. 2, 9, 13. But this stele has the figure of a man holding a long pike. I have assumed, therefore, that *δοτ-* corresponds to *σύμβιος*, and that it might ordinarily be translated "wife" since the husband more often erected a monument to his wife than the reverse; but that here the wife was erecting a monument to her warrior husband, and we should have to translate "husband, spouse." But actually the word in this inscription reads *δ.τες*, as the first vowel, occurring at the end of the first line, is lacking. And we shall find at the end of this paper that "Pisidian" employed vocalic alternation; so we are not sure that *δ.τες* "for husband" did not have another vowel than *δοτες* "for wife."

This series of inscriptions that we have just been considering, nos. 5, 7, 3, containing the word *μονογήτη* and an indirect object, are peculiar in their lack of a subject—the name of the tomb builder. Yet no. 5, interpreted "Stele for Ta," is complete enough, corresponding to 'Ε]πὶ Ἀττάλω ἵρωι at Tlos. But no. 7, interpreted as "Stele for son," and no. 3, "Stele for husband," do not identify the person sufficiently—do not tell us whose son or husband he was, or his name. Now while we might be wrong in interpreting *γδασας* as "for son," instead of "for Gdasa," we can hardly be wrong here in interpreting *δοτες*, if such it is in no. 3, as "for spouse." I believe the identification in these inscriptions came from the engraved figure. While probably not very lifelike, it no doubt had a symbolic value, like the "Sign of the Blue Boar." Thus, in this rather primitive community, while more than one man had probably carried a pike, there may have been only one of them noted enough to have a monument, and on looking at it the inhabitants knew at once that it was the grave of one of their famous warriors. Similarly in no. 7, there may have been a significant mark on the figure which distinguished the youth from other persons' sons.

2. Br.: "Der Lir wurde das Denkmal aufgestellt."
6. Br.: "Für den Gpuroxas (ist es) ein Andenken (Denkmal)."
1. Br.: "Für den Dotaris soll das Denkmal sein. Dotaris (der Sohn und) des Dotaris Tochter." Again Brandenstein, as

in no. 4, must divide the short inscription into two disjointed sentences, of which the second is a kind of signature—contrary to most epitaphs of the region. He still must supply words to make any sense at all.

Yet this inscription was the most difficult of all for me to interpret, and it is only due to Brandenstein's translation of the last *δωραπι* as a genitive that I was able to make a satisfactory rendering: "Dōtari made this for Dōtari (and) for the children of Dōtari," assuming that one man and one woman shown in the figures were the grown children of Dōtari. This would make the genitive the same as the nominative—as the Greek transcription has rendered them. The two case endings may have been primitively different and have fallen together. Or the genitive may differ from the nominative in accent or by the addition of a phoneme which could not be represented in Greek. The most common Lycian genitive termination is + which has been transcribed *h*. This phoneme is not written in Greek transcription; thus in the Lycian-Greek bilingual (Kalinka no. 56) the genitive *hla-h* of the Lycian personal name *hla* is transcribed Αα in Greek.

9. Br.: "Der Ias hat (es) die Nis aufgestellt." Brandenstein's division of *ειανις* into *εια* and *νις* is supported by *εια* alone occurring as a personal name in the nominative in no. 14, and the division may be correct. If so, my translation should read "Eia for Ni, (his) wife." But then we probably should divide *ονανις* in no. 10 into *ονα* and *νις*, "Oua for Ni," which does not make sense in the context. Hence I have considered *ειανις* and *ονανις* to be single words.

10. Br.: "Für den Menouas das Grab (und) für die Wanis das Grab (ist es)." Brandenstein did a little silent violence to this inscription in omitting the final -s of *ονανις* and in the correction of [β]αβ[ον] to γδαβος without comment, to make this inscription fit the model of no. 11. The omission of -s is unwarranted, as Ramsay cast no doubt on the reading of the letter. Ramsay thought βαβον might be the name of Ouani's father, Babas, a very common name in Pisidia and Phrygia. Without a photograph of the stele, speculation on the restoration of [β]αβ[ον] and the precise interpretation of the inscription seem futile to attempt.

The γδαβος which does occur in the inscription corresponds

formally to *μονογήτος*. I have tentatively translated it "erected" primarily to distinguish it in translation from *μονογήτος*, secondarily because *ἀνέστησεν* probably occurs next most often to *κατεσκεύασεν* in the Greek inscriptions of Pisidia and vicinity.

11. Br.: "Für den Gas das Grab (und) für die Edas das Grab (ist es)." Here again Brandenstein must supply the verb, which is not necessary in my interpretation.

13. Br.: "Für den Menis wurde (ein Grab) gegraben (und) die Ena-ouper hat (es, das Grabmal) aufgestellt."

We may best take up here my interpretation of *τι* and *δοτ-*. In the Greek epitaphs of Lycia which provided *for* (dative) the family,<sup>37</sup> the order of mention was "self" 63, "wife" 40, "children" 29, "those from themselves" 17; and Lycian *for* (dative) formulas or thereafter: "self" (*hrppi*) 57, "wife" (*ladi*) 48, "children" (*tideime*) 45, "those from themselves" (*esedēñnewi. -e*) 7. This is what we might expect. The tomb owner would naturally look out for himself first; death will sometimes have removed the spouse, so that this word will occur less often; sometimes the tomb owner will not have had children, or they may have died, been adopted, or set up establishments of their own.

We should expect "self" and "wife" to be expressed several times in the "Pisidian" inscriptions, and we find *δοτ-* and *τι-* four and three times respectively, and we should expect on a mathematical basis that *δοτ-* means "self" and *τι-* "wife." While this would fit grammatically and would accord well enough with the figures engraved on the stones, it would be contrary to reality; for it would have two women erecting two tombs to themselves (nos. 2, 9) compared to one man erecting a tomb for himself (no. 3), while a preponderance of male to female tomb builders is usually found. And in this inscription, no. 13, the interpretation of *δοτ-* as "self" and *τι-* as "wife" would make the tomb owner mention his wife before himself, which is contrary to custom. Actually there are ten figures of women engraved on the steles compared to those of nine men, so that we may infer that by accident there were more wives to

<sup>37</sup> I. e., those formulas which employ a dative—instead of a verb expressing "bury" before the list of persons to occupy the tomb. The former is much more common than the latter.

dispose of than men to make provisions for their own burials. And we may conclude that δοτ- means "spouse" and τι- "self."

The meanings of *eva* and *ouπερ* were arrived at by the combinatorial process and have nothing else to support them. One may compare Gr. ἵπέρ with the latter.

14. Br.: "Für die Ias wurde es gegraben."

12. Br.: "In dem Grabe soll Menes begraben sein." As γδεβε is a verb, which I have translated "erected," γδαβα must be a verbal noun, let us say "monument." As -s is the inflection of the dative, μηνες must mean "for Mēnei," the same personal name that we saw in no. 13 above; and as we saw that τις means "for himself," τιε must be in some other case than the dative. Owing to these considerations, "Monument for-Mēnei erected he-himself" seems the only possible interpretation, and γδαβα would necessarily be accusative. This is the only instance where an accusative is required if one interprets μονσητα as well as μονσητος as a verb, "made."

We have not yet considered the very short "Pisidian" inscriptions which Brandenstein and I agree consist of personal names. Two of these are:

15. τ. ερινεις

For T.erinei

1 m.

16. νηλικας

For Neliaka

1 w.

Brandenstein: (15) "Dem -t.er (hat es aufgestellt) Inis"; (16) "Der Nelis (hat es aufgestellt) Kas."

From my interpretation, these inscriptions would roughly parallel 'Ε]πὶ Αττάλω ἥρωι (Tlos, Kalinka 635). We know so little about "Pisidian" personal names and compounds that I hardly feel warranted in dividing these inscriptions into two words. Yet I think Brandenstein's separation may have some merit. If so, I should translate no. 15 as "T.er, for-son"; cf. *ιεις* with *ειεις* in no. 1. And no. 16, "Neli, for-Ka"; cf. type 2 epitaphs under Sagalassus above.

The remaining "Pisidian" inscription is

8. σλπουροξα

Slpouroksa.

1 w.

Brandenstein translated this "Für die Slpouroxas," merely remarking "ein Nominativ wäre wohl recht unwahrscheinlich." Perhaps Brandenstein had some reason—some Austrian or other tombstones with which he was familiar—for thinking the nominative unlikely. The nominative does not impress me as peculiar. Very short epitaphs are not numerous in Anatolia, but among them nominatives are not unknown, as at Zengibar Kalesi (Palaia Isaura)<sup>38</sup> and at Cadyanda (Lycia).<sup>39</sup> Short inscriptions are too few to make a categorical distinction, but the general rule in Lycian appears to have been that names distinguishing engraved figures on a monument were in the nominative (generally in -a) hellenized in Greek transcription with the addition of -s, at least to masculine personal names, as in Lycian 32, 125, and 24 (*izraza*) ; elsewhere Lycian employed the genitive.

The genitive was more common in Lycia and occasionally elsewhere in epitaphs consisting solely of a personal name, as at Telmessus,<sup>40</sup> Xanthus,<sup>41</sup> Pinara,<sup>42</sup> Cyanis,<sup>43</sup> Araxa (Ören),<sup>44</sup> and Zengibar Kalesi (Palaia Isaura).<sup>45</sup> And at Cyanis, no. 69, *ipresidax armpax tideimi tuburex*, "Of Ipresida Armpa, son of Tubure"; and at Limyra, no. 129, *hlah*, "Of Hla"; no. 130, *ddepñneweh*; no. 141, *midah*.

Such evidence as we have from comparison of epitaphs indicates that "Pisidian" σλπουροξα may be either a nominative or a genitive. As -a is the most common termination of the nominative, but here may be a genitive ending, this adds to the possibility that "Pisidian" stems, as written in Greek transcription, had the same endings in the nominative and genitive singular;<sup>46</sup> see discussion of δωταρι above under no. 1.

<sup>38</sup> Nos. 175, 178, 180, 188, 200, 201.

<sup>39</sup> No. 696. Also at Limyra, no. 143, *Koðapas Oσalμιος* (hellenized with -s in N.), and no. 134, *Maσa Koata* (native, without -s in N.).

<sup>40</sup> Nos. 30, 31, 32.

<sup>41</sup> No. 313.

<sup>42</sup> 514, 515.

<sup>43</sup> 72.

<sup>44</sup> 705.

<sup>45</sup> 181.

<sup>46</sup> The Termessian names with N. -as and G. -a noted by Niemann and Petersen, *op. cit.* (in n. 11 above), p. 30, may similarly have had simply -a in the native language in both cases, -s having been added in the Greek transliteration to distinguish the nominative.

## “PISIDIAN” GRAMMAR.

*Phonetics.*

“Pisidian” employed all the letters of our standard Greek alphabet except  $\zeta$ ,  $\psi$ ,  $\chi$ ,  $\theta$ ,  $\phi$ . The lack of  $\zeta$  and  $\psi$  probably should not be interpreted as indicating that these affricates were absent in the language but only that by accident they do not occur in our limited materials; for  $\xi$  occurs twice. But the lack of all three Greek aspirated stops in the “Pisidian” texts does indicate their probable absence, as in Phrygian.

We have too few different stems to state what phonemes could stand initially, but as we have the complete dental series ( $\tau$ ,  $\delta$ ,  $\nu$ ) and scattered representatives of other consonantal series and of the vowels, it seems likely that most phonemes, at least, could stand initially.

But, to judge from our materials, words could end only in a vowel (including diphthongs and perhaps triphthongs), *r*, and *s*. This is not too dissimilar from New Phrygian—in which the finals appear to have been vowels (including diphthongs and triphthongs), *s*, *n*, and *t*—when we consider that “Pisidian” may have lost a final nasal in its accusative singular, as suggested above, and that Phrygian *-t* probably only occurs in a verb ending, for which we may not have the corresponding form in the “Pisidian” texts.

Another feature of “Pisidian” phonetics is the number of written diphthongs and triphthongs. Some of these triphthongs of which *ov* is a component, as *ova*, probably only represent diphthongs phonetically, and similarly *ea* may represent only *ia* phonetically. But *ao* in *taos* is probably a true triphthong.

Vocalic alternation is one of the chief peculiarities of “Pisidian.” Some of these, such as *μονσητα* (nos. 4, 5, 7) but *μοσητα* (no. 3), or *μονσητος* (nos. 6, 2) but *μοσητως* (no. 1), may represent the inadequacy of Greek to transcribe “Pisidian,” or may reflect Greek dialect variants, “Pisidian” dialect variants, or phonetic shifts. But  $\gamma\delta\alpha\beta\sigma$  and  $\gamma\delta\alpha\beta\alpha$  compared to  $\gamma\delta\epsilon\beta\epsilon$  could be attributed to none of these, but must represent a morphophonemic process such as occurs in Indo-European and Semitic languages.

Another notable phonetic phenomenon in “Pisidian” is the

occurrence of initial phonetic clusters, *gd-*, *gp-*, *slp-*.<sup>47</sup> Sometimes, at least, these appear as the result of consonantal prefixation, as *γ-πονρωξα* compared to *σλ-πονροξα*. As the last is probably feminine, but *γ-πονρωξο* probably masculine, and as *γ-δασας* occurs on a stele with a relief representing a young man, we may have an indication that sex was denoted by prefix rather than by suffix. Brandenstein thought that masculine and feminine were not distinguished by the endings—a view I also share, although on other grounds and the evidence is not yet overwhelmingly conclusive. Initial consonantal clusters are not found in Phrygian.

#### *Inflection.*

Nouns.—The nominative probably consists of the stem; cf. *Δωταρι* (N.) but *Δωταρις* (D.) in no. 1. Note also the nominative *Λιρ* which certainly looks like a stem in *-r*. Generally the nominative ends in *-a* as in Lycian.

The genitive seems to be the same as the nominative, if Brandenstein's interpretation of the second *Δωταρι* in no. 1 as a genitive is correct, as I think probable. Also *σλπονροξα* may be a genitive, although *-a* is generally the ending of the nominative in our texts.

The dative ends in *-s*. Usually this consonant is added directly to the stem, as N. *Δωταρι*, D. *Δωταρις*, and probably in the datives *τα-s*, *γδασα-s*. But this is not always true, for we have the N. *μηνει* in no. 13, but the D. *μηνες* in no. 12. And if my interpretation is correct, we have a nominative *τιε* of the reflexive pronoun, but D. *τι-s*. We also have as datives of the same root, *δοτεις* and *δοταις* with no apparent difference in meaning. The difference in spelling may only represent the inadequacy of Greek to transcribe a single "Pisidian" phoneme, or a "Pisidian" phonetic shift *ai* > *ε*, or *ε* > \**ει* > *ai*. Yet it has long been recognized that Lycian has two accusative singular endings,

<sup>47</sup> For some of the phonetic features of "Pisidian" considered here, cf. the remarks of Niemann and Petersen, *op. cit.*, pp. 10 ff., on the "Sprache der Pisidier" as revealed in Pisidian Greek inscriptions; and G. Radet, "Les villes de la Pisidie," *Rev. Arch.*, 3d ser., XXII (1893), p. 213. For consonantal clusters, see Kretschmer, *Einl.*, top p. 347. A different type of cluster, *st* + semivowel, seems to occur in Selge: ΣΤΑΓΕΙΤΣ, ΕΣΤΑΓΕΙΤΣ, etc. (on coins), and at Aspendus: ΕΣΤΦΕΔΙΠΝΣ, -ΤΣ.

-ā and -u, which have never been satisfactorily explained; their use throughout Lycia, in the same towns and even on the same monument—as *prñawā* and *ladu* on no. 91, which was clearly inscribed at one time—make dialect variant or phonetic shift impossible solutions; the context and the syntax likewise does not seem to affect the endings; so we seem to have two accusatives in Lycian, differing in a manner which Greek could not express. The two “Pisidian” datives -es and -aus which we find with the stem δοτ- may be similar, although a phonetic solution seems more probable for “Pisidian” datives at present in view of apparent phonetic irregularities elsewhere in the language.

The accusative, as written in Greek letters, probably was the same as the nominative; see the discussion of no. 4 above.

We have no examples in the plural.

*Verbs.*—I have translated the verbs in the past tense, as they probably correspond to the Greek aorist usually found in epitaphs to express the building or erection of the monument. I have suggested that γδαβος is an ordinary 3d sg. past tense, while γδεβε may be a 3d sg. of a sort of reflexive past. It has also been suggested that μουσητα and γδαβα were verbal nouns or past participles. An alternative, however, may be that μουσητα is a form of the past of a “weak” verb, and γδεβε of a “strong” verb.

ROBERT SHAFER.

## SOME EPIGRAMS BY LEONIDAS OF TARENTUM.

"Martial has wit, and is worth your looking into sometimes; but I recommend the Greek epigrams to your supreme contempt." So wrote Lord Chesterfield to his son on January 25, 1745, advising him to get out of that "worst company in the world." Today we no longer shun the Greek epigrammatists: they have become respectable, and we admire their poems, realizing that they are not inferior to those of Martial, but different, and that they must not be judged by criteria imposed by a later concept of epigram. But while it has recovered from one prejudice criticism seems to have succumbed to another; at any rate comments and judgments of editors still betray bias and narrowness of concept. Witness, for example, Mackail (*Select Epigrams* [London, 1906], p. 5): "No good epigram sacrifices its finer poetical substance to the desire of making a point; and none of the best depend on having a point at all"; or Leslie Shane (*The Greek Anthology* [New York, 1929]): "With Simonides the Greek epigram reached an immediate height, to which it never re-ascended"; or, more recently still, F. L. Lucas (*A Greek Garland* [Oxford University Press, 1939]), who in outlining the history of the *Palatine Anthology* manuscript makes the revealing comment: "These relics of dead antiquity, these quiet epitaphs on the fallen of Thermopylae and Plataea, have continued to be hurled this way and that by the earthquakes of European history." A less adequate description of the *Anthology* than "these quiet epitaphs on the fallen of Thermopylae and Plataea" could hardly be imagined. We are all too easily tempted, in fact, to view ancient epigram in the light of its extremes, to contrast the simple austerity of Simonides with the brilliant pointedness of Martial.

Concepts so narrow must naturally exclude almost all the poems in the *Palatine Anthology*: the holders of such views apparently fail to realize that the ancient epigram—whether the term *ἐπίγραμμα* be used or not—was not a static and achieved form, but dynamic and variable. One particularly interesting development is found in a group of poems by Leonidas of Tarentum: these lie midway between the two extremes; for the

most part they preserve the sepulchral and dedicatory tradition of early epigram, while at the same time, almost four centuries before Martial, their rhetorical structure is such as to make the witty point which the modern concept of epigram demands. True, they leave no hurt: but surely wit is not necessarily bitter, nor need the point be a sting.

Our examination of Leonidas' work may begin with a single distich, *Anth. Pal.*, VII, 452—

*Μνήμονες Εὐβούλοι σαόφρονος, ὡ παρεόντες,  
πίνωμεν· κοινὸς πᾶσι λυκὴν Ἀΐδης<sup>1</sup>*

—a simple little thing in which the tone is set by *μνήμονες* at the start; a sepulchral epigram of sorts that might be translated casually and left just as casually: “In memory of prudent Eubulus, all that are present, let us drink; Hades is an anchorage common to all.” We soon notice, however, that *σαόφρονος* seems to bear a certain emphasis, since it ends a *κῶλον* at the bucolic diaeresis, and that *ὡ παρεόντες* has a kind of parenthetical, *sotto voce* effect. Then, as we reach the noble Alcaic *πίνωμεν*—a familiar initial utterance—we begin to wonder: Is this the kind of remembrance act that we expect; and how was Eubulus *σαόφρων*; was he a teetotaler; and what about his name, “Mr. Wiseman”; was he really a wise man? A man with such a name might reasonably be expected to plan his life wisely; but to what avail? All he stood for is crushed by the concluding irony—where, too, the keynote *κοινός* is emphasized by its metrical position: “It's all the same; all of us—sober and drunkards alike—come to rest in the same harbor in the end.” A further point may be added: among the Dorians of Sicily *μνάμων* was used to denote the *magister convivii*; a Tarentine poet's use of *μνήμονες* might well evoke this connotation; we should then

<sup>1</sup> The text at the start of this poem is suspect; I have preferred Casaubon's emendation of the MS *μνήμης*, but the general sense of the poem remains the same whatever reading is chosen: *μέμνησθ'* is read by W. R. Paton in the Loeb edition. The reading *παρεόντες* is Wilamowitz' emendation of the MS *παριόντες*; to retain the MS reading would tend to eliminate the sympotic element and would raise the question, In what circumstances would passers-by be able to drink a toast? Apart from this the readings adopted are those of Edwyn Bevan in his edition of Leonidas' poems (Oxford, Clarendon Press, 1931); unless it is otherwise indicated, his text agrees with that of Paton.

have a skilful contamination of the sepulchral epigram with sympotic elegy. Perhaps something like this may give the effect:

Remembering Wiseman's temperate ways,  
Friends gathered here, I bid you cry  
“A toast!” Death's haven waits for all,  
Both wise and foolish, wet and dry.

Another sepulchral epigram, *Anth. Pal.*, VII, 740, which brings its subject into prominence by the use of his name in identical metrical positions in the first two verses, allows the poet to display other technical tricks:

Αὗτα ἐπὶ Κρήθωνος ἐγὼ λίθος, οὖνομα κείνου  
δηλοῦσα, Κρήθων δὲ ἐγχθόνιος σποδά,  
ὁ πρὸν καὶ Γύγη παρισεύμενος ὅλβον, ὁ τὸ πρὸν  
βουνάμων, ὁ πρὸν πλούσιος αἰπολίοις,  
ὁ πρὸν (τί πλείω μυθεῦμ' ἔτι;) πᾶσι μακαρτός,  
φεῦ, γαῖης ὄσσης ὄσσον ἔχει μόριον.  
2. ἐν χθονοῖς Paton

Once more the value of the bucolic diaeresis makes itself felt by the contrast that results from the juxtaposition of *ἐγὼ λίθος* and *οὖνομα κείνου*—in many respects an epitome of the whole poem, something solid and permanent contrasted with something shadowy and transient. And, in defiance of grammatical structure, I feel that the lines read: “Here above Crethon I stand, a stone, while he is but a name, an inscription ‘Crethon,’ no more than dust in the ground.” To me the very rhythm of *δηλοῦσα*, *Κρήθων* suggests quotation marks around the name.

Verse 3 introduces us abruptly to the transient part: before a half verse is over we can see that Crethon was once a veritable Gyges—and it may be noted that *Γύγη* is metrically equivalent to *Κρήθων*—in verse 1. Verses 3-5 are devoted to his past glories, and all kinds of changes are rung on the words *ὁ πρὸν* and, at the same time, on the grammatical scheme of the descriptive phrases. The thesis-arsis form *ὁ πρὸν* is replaced by *ὁ τὸ πρὸν*, with *ὁ* relegated to the arsis of the preceding foot; then replaced by the arsis-thesis *ὁ πρὸν* of verse 4, until in verse 5 we return to the original form, which produces a neat anaphora to verse 3 and rounds out the triple group of verses. Meanwhile the aspects of Crethon's wealth are denoted by a participle with an

accusative, a single descriptive adjective, an adjective with a dative, and finally another adjective with a dative of a different type. One may perhaps suggest that interest in Crethon's early state is kept alive by the metrical pause between ὁ πρίν and πλούσιος in the middle of verse 4, and certainly the bucolic diaeresis and the parenthesis of verse 5 give unusual force to the all-embracing πᾶσι μακαρτός. No wonder the poet cried φεῦ as he reached the climax, and—significantly—it is ground, the earth of verse 2, that is mentioned. We may note in passing the collocation with different metrical accents of ὅσσης ὅσσον. The poem ends: φεῦ—what an expanse of land! And to what are we reduced now? μόριον. Of all those acres, only six feet now!

I stand on Crethon's grave, a stone, while he  
 Is but a name inscribed and dust interred.  
 In wealth he rivalled Gyges once; he once  
 Was rich in cattle, once had prosperous flocks—  
 Need I say more?—once envy of all! And oh!  
 The land he owned! His plot is six feet now.

An even less pretentious plot of ground seems indicated in *Anth. Pal.*, VII, 656:

Τὴν ὄλιγην βῶλον καὶ τοῦτ' ὄλιγήριον, ὥνερ,  
 σῆμα ποτίφθεγξαι τλάμονος Ἀλκιμένευς,  
 εἰ καὶ τāν κέκρυπται ἵν' ὀξείης παλιούρου  
 καὶ βάτου, ἦν ποτ' ἐγὼ δῆιον Ἀλκιμένης.

Here the metrical parallelism and assonance of τλάμονος Ἀλκιμένευς and δῆιον Ἀλκιμένης are obvious; but the combination of τλάμονος with the heroic-sounding name Alcimenes—*Mr. Standfast*, one might say—perhaps prepares us for the final effect of the heroic word δῆιον; the combination of this word with Ἀλκιμένης in the effective finale of the poem, contrasted with the humble tone of verse 1, creates the ironic climax which I try to suggest in my translation.

This tiny plot and tiny stone, good sir,  
 Deserve respect; they mark the grave of one  
 Who had a steadfast heart, Alcimenes,  
 Though buried now beneath my foes, brambles  
 And crooked thorns, which, living, I laid low  
 In gallant fight, a true Alcimenes.

A similar, though less disputable irony, occurs in *Anth. Pal.*, VII, 455:

Μαρωνὶς ἡ φίλουνος, ἡ πίθων σποδός,  
ἐνταῦθα κεῖται γρῆνς, ἡς ὑπέρ τάφου  
γνωστὸν πρόκειται πᾶσιν Ἀττικὴ κύλιξ,  
στένει δὲ καὶ γᾶς νέρθεν, οὐχ ὑπέρ τέκνων  
οὐδ' ἄνδρός, οὐδὲ λέλοιπεν ἐνδεεῖς βίου,  
Ἐν δ' ἀντὶ πάντων, οὐνεχ' ἡ κύλιξ κενή.

The extravagance of Maronis' passion is already displayed in the first verse; she must have been little more than a walking wine-jar in life, for she is now reduced to dust like that of such a container. The image persists in *κεῖται* (*stored up*)—though, of course, this is also the usual word for the position of a corpse; I should like to think that it is continued in the word *γρῆνς*, though the word usually applied to wine is *παλαιός*. Perhaps we should see it, too, in the phrase *γᾶς νέρθεν* (which suggests the storage of the jars). The verb *κεῖται* is picked up in *πρόκειται* of verse 3—at once a metrically equivalent sound reminiscence and a recollection of the *πρόθεσις*, the ceremonial laying-out of the corpse. One wonders if Maronis is seen not only in terms of the wine-jar, but also as the drinking-cup, which is displayed to view as once Maronis' corpse was laid out in burial for display. Another sound reminiscence is the *ὑπέρ τέκνων* of verse 4, which picks up metrically as well as acoustically the *ὑπέρ τάφου* of verse 2, yet with a different meaning of *ὑπέρ*—and this difference is emphasized by the *νέρθεν* which precedes *ὑπέρ τέκνων* and is in direct contrast with the *ὑπέρ* of verse 2. Then in verse 6 the point made in verse 1 is pressed home: Maronis is dead and, although she has imbibed all the resources of the household, has few regrets; she has a drinking-cup near at hand, but, alas!—the cup is useless, it is forever empty!

Maronis, wine-sot, now mere wine-jar's dust,  
Lies here, well-aged; and o'er her tomb is set  
A well-known sign—an Attic drinking-cup.  
She mourns below, not o'er her children's fate  
Nor husband, whom she left in poverty;  
There's but one grief—the cup is never filled!

One type of epigram which became popular in the early Hellenistic period was the poem which praised the achievements of some contemporary or earlier artist—sculptor, painter, or

writer. Many of these which commended sculptures or paintings were akin to the original epigram, the inscription, while many of the poems praising the works of writers may have been inspired by and served—in more elaborate form—as identification or catalogue tags for rolls of manuscript. Such a poem is *Anth. Pal.*, IX, 25, written by Leonidas for his contemporary Aratus:

Γράμμα τόδ' Ἀρήτου δαήμονος, ὃς ποτε λεπτῆ  
φρόντιδι δηγαιοὺς ἀστέρας ἐφράσατο,  
ἀπλανέας τ' ἄμφω καὶ ἀλήμονας, οἵσιν ἐναργῆς  
ἰλλόμενος κύκλοις οὐρανὸς ἐνδέδεται.  
αἰνεῖσθω δὲ καμῶν ἔργον μέγα καὶ Δίὸς εἶναι  
δεύτερος, ὅστις ἔθηκ' ἀστρα φαεινότερα.

Within the first line the significantly placed words *δαήμονος* and *λεπτῆ* bestow on Aratus a compliment which most Hellenistic writers would have been eminently happy to receive, commendation for his learning and for his nicety of perception and expression. Certain sound effects apparently interested the poet—a preponderance of *a*'s and repetition of the sound and rhythm of *δαήμονος* in *ἀλήμονας*; there is a hint of oxymoron in the “binding fast” of the “whirling firmament,” and, I believe, anticipation of the final word of the epigram in the use of *ἐναργῆς*, which I take to mean “brilliant” here. But, to my mind, the weight of the poem seems to be concentrated chiefly in the last couplet. With the phrase *καμῶν ἔργον μέγα* we stand squarely in the middle of the Alexandrian world of literary criticism. In *καμών* is reflected Alexandrian insistence on laborious painstaking effort, that element which we ourselves popularly describe as nine-tenths of genius; this is what Lycidas—sometimes identified with Leonidas—was referring to in Theocritus' Seventh Idyll when he told his friendly rival that he would sing a song which he had elaborately worked out (*ἔξερπνασα*); it is the *ἄγρυπνίη*, the burning of the midnight oil, which, says Callimachus in his epigram on Aratus, produced the *Phenomena*. The words *ἔργον μέγα* must have been like our proverbial “drop of a hat”; they would provoke a dispute in almost any Alexandrian circle; to devotees of the “path untrodden by others, even though it be a narrower one,” to those who preferred the “clear spring” to the “muddy river's flow,” a *μέγα βιβλίον* was, as Callimachus phrased it, *μέγα κακόν*. And

yet Aratus had produced an *ἔργον μέγα* while displaying that quality of *λεπτότης, finesse*, which some had thought incompatible with a *μέγα βιβλίον*. His achievement was inferior only to that of Zeus; only one poet held an unapproachable, incomparable eminence similar to that of Zeus—Homer whose apotheosis is in fact recorded in the Hellenistic relief by Archelaus. The Hellenistic poets, acknowledging Homer's inimitable position, took as their model Hesiod, the didactic hexameter poet. Now Callimachus said that Aratus' poetry was Hesiodic in tone; may we suppose that Leonidas, in ranking Aratus next to a Zeus-like Homer, reckoned him comparable with his model Hesiod?

One further point: in view of the title of Aratus' work, *Phenomena*, the last word of the epigram seems to me not without point; Zeus—as Aratus often points out—was inspired by his care for men to create the “phenomena”: Aratus made them even more “phenomenal.”

Aratus wrote this work; in it he joined  
Learning and sensitivity of mind,  
As he described the ever-living stars,  
The fixed stars and the planets, too, which lend  
The circling heavens their brilliance and hold fast  
The restless movement of the firmament.  
Commend him for the mighty work on which  
He toiled; acknowledge him as close to God  
Because he made God's stars more lustrous still.

A different, almost Restoration cavalier tone characterizes *Anth. Pal.*, V, 188:

Οὐκ ἀδικέω τὸν Ἐρωτα· γλυκύς, μαρτύρομαι αὐτὴν  
Κύπριν· βέβλημαι δ' ἐκ δολίου κέραος,  
καὶ πᾶς τεφροῦμαι· θερμὸν δ' ἐπὶ θερμῷ ίάλλει  
ἄτρακτον, λωφῷ δ' οἰδ' ὅσον ιοβολῶν.  
χώθηντὸς τὸν ἀλιτρὸν ἔχω, καὶ θηητὸς ὁ δαίμων,  
τίσομαι· ἐγκλήματα δ' ἔσσομαι ἀλεξόμενος;  
5. ἔγώ, καὶ πτηνὸς Paton

I have tried to adumbrate some of my impressions of the word order in this poem in the translation which I offer. But the most striking feature of the epigram is the uniform firmness with which the poet drives home the point of his innocence. His defense is offered specifically in three almost identical

metrical phrases concluding the three couplets of the poem; the elided δ' in each case can hardly escape notice, and in the first phrase *βέβλημαι* bears all the emphasis that such a combined rhetorical and metrical position can offer—final in its κῶλον, initial in its sentence: Leonidas is the victim, the aggrieved party, not the aggressor. Perhaps his exasperation shows itself in the word *τίσομαι*—initial in its κῶλον, final in the sentence, and the only dactyl in this position. But surely extraordinary emphasis falls on the last word of the poem, ἀλεξόμενος; aggressor or not, *here* lies the poet's basic claim for his initial utterance “Not guilty.”

“Not guilty of hurt to Cupid” is my plea;  
 Venus can tell, my character is mild.  
 No, I'm the victim of his treacherous bow.  
 And now, from head to foot, I'm all aflame;  
 Burn after burn I suffer from his shafts.  
 And respite?—No, his arrows never cease.  
 I've caught the rascal god, and if—like me—  
 He feels a mortal wound, I'll have revenge.  
 Indictable?—Oh, no! It's self-defense!

The last poem of which I offer any detailed analysis is *Anth. Pal.*, VII, 316:

Τὴν ἐπ' ἐμεῦ στήλην παραμείθεο, μήτε με χαίρειν  
 εἰπών, μήθ' ὄστις, μὴ τίνος ἐξετάσας·  
 η̄ μὴ τὴν ἀνύεις τελέσαις ὁδόν· ἦν δὲ παρέλθης  
 συγῇ, μηδ' οὐτως ἦν ἀνύεις τελέσαις.

These words are ascribed to Timon the Misanthrope and are eminently appropriate to his character. Wayfarers are usually expected to stop and pay their respects at a grave. This courtesy is expected to have a kindly reward. Timon naturally does the reverse. But, before we investigate Timon's view of the practice, we should note the poet's literary artifice.

Noticeable are the changes rung on the negative μή—μήτε in verse 1, μήθ' and μή in verse 2, μή in verse 3 and μηδ' in verse 4;<sup>2</sup> the alternation of ὄστις and τίνος in verse 2 and the repetition of τὴν ἀνύεις τελέσαις ὁδόν in a different metrical position with substitution of η̄ν for τὴν and omission of ὁδόν are characteristic of a period interested in *ποικιλία, variatio*. More worthy of note

<sup>2</sup> Another instance of the poet's interest in sound patterns is suggested by the preponderance of the sounds η and ει.

is the similarity in metrical position and rhythm between *εἰπών* and *σιγῆ* and their obviously emphasized contrast in meaning.

But most significant, and thoroughly characteristic of the person commemorated, is the general sentiment of the poem. As I have said, a wayfarer's courtesy would presumably be rewarded; in the case of Timon, however, cynic and misanthrope, the reward might well be unexpected, the reverse of normal. He will have no truck with humankind: he disliked their company when alive and will have none of it now. Then, suppose one disregards Timon?—and it is difficult to imagine a more emphatically shaped clause than *ἢν δὲ παρέλθης | σιγῆ*—surely one may depart unmolested. No, one is no better off! All men, courteous and discourteous or cautious alike, meet with the same curse.

Pass by my tomb, and greet me not, nor ask  
“Whose grave?” “Whose son lies here?” or else I hope  
You never reach your goal. But pass in quiet,  
Why, then, I hope you—never reach your goal!

The suggestions I have made for the interpretation of these poems may, perhaps, vary in plausibility, but I feel that careful study reveals convincingly that Leonidas fully appreciated how valuable for purposes of effective emphasis the last verse, last clause, or even last word could be.

Leonidas was, of course, by no means the first to realize the rhetorical possibilities inherent in the final part of a short poem. The quatrain in which Archilochus tells how he lost his shield in battle comes close to the later type of epigram with its unexpected turn of thought at the end; with a professional soldier's contempt for the conventions of ancient knighthood the poet concludes: “To the devil with it! I shall soon get another just as good.” Occasionally we meet a sepulchral epigram in which the final word seems particularly well chosen, as in Simonides, 115 (Diehl) : *κάλλιστον δ' ἀρετῆς μνῆμ' ἔλιπον φθίμενοι*—“Outstanding was the valor they commemorated by their death.” To Demodocus, whose “Cappadocian bitten by a snake” (4 Diehl) anticipated Goldsmith's well-known “The dog it was that died,” is attributed a veritable epigrammatic formula which may be represented by 3 (Diehl) :

“Cilicians all are sorry folk, of very ill condition  
Save Cinyras; and Cinyras is, after all, Cilician” (Leaf).

Here we have the ancestor of Porson's "And Hermann's a German!" But such poems occur only sporadically in the preserved Greek literature of the three centuries before Leonidas.

Yet this was apparently not true in the case of the *ἐπίγραμμα* proper, as can be seen from a study of the inscriptions published by Paul Friedländer and Herbert B. Hoffleit in their *Epigrammata: Greek Inscriptions in Verse from the Beginnings to the Persian Wars* (University of California Press, 1948). Hexametric, elegiac, and iambo-trochaic inscriptions, from periods as early as 600 B.C., and from various parts of the Greek world, frequently show a fondness for final rhetorical emphasis. The amoral ruthlessness of the sea is suggested by Δεινία τόδε [σῶμα], τὸν ὥλεσε πόντος ἀναι[δής] (*op. cit.*, no. 2: Corinth, ca. 600 B.C.). In no. 24 (Corcyra, ca. 600),

[Στάλα Ἐν]τίμου ματρὸς ἐγὼ ἔστακ' ἐπὶ τύμωι  
Πόλυνόφας· σ[τοναχὰ δ' νιῶι κατελεῖ]πετο ματρ[ός]

the repetition of *ματρός* or a cognate form is surely much more than the amplification suggested by the editors: it emphasizes the grief of this mother's son. A similar effect is perhaps created by the *ματήρ* at the end of verse 1 and the final *ἐπέθηκε δὲ παιδί* in no. 30 from Troezen.<sup>3</sup> The prayer found in an inscription from Crissa (no. 44) could hardly be more strongly expressed within the limits of a single distich:

Τάσδε γ' Ἀθαναίαι δρα[χμὰ]ς Φαγάριστος ἔθηκε  
"Ηραι τε, ως καὶ κείνος ἔχοι κλέφος ἀπθιτον αἴρει

and, if the reconstruction of no. 87 (Athens) is correct, the valor of Xenocles is firmly brought to the reader's attention by the final position of *ἀρίστας* and *ἥντρέας* in verses 1 and 2. Another dedicator made his desires clear in an inscription from Lucania (no. 111, 6th century):

Χαῖρε, φάναξ "Ηρακλες· ὁ τοι κεραμεύς μ' ἀνέθηκε·  
δὸς δέ φ' ἵν ἀνθρώποις δόξαν ἔχειν ἀ<γ>αθ<ά>ν

while the tombstone commemorating Tettichus, ἄνδρ' ἀγαθόν (no. 135: Attica, ca. 550 or earlier), used the final words of its inscription to urge its readers to follow Tettichus' example—*νεῖσθ' ἐπὶ πρᾶγμ' ἀγαθόν*. Number 146, derived from Herodotus,

<sup>3</sup> Cf. also no. 32 from Oloosson in Thessaly.

IV, 88 and celebrating Mandrocles' bridging of the Hellespont for Darius, ends with the words Δαρέον βασιλέος ἐκτελέσας καὶ νοῦν; the editors justifiably remark (p. 138): "It is symbolic of the historic position of the Ionians that this poem, very Greek in its clear simplicity and its joy of action and renown, culminates in the will of the Persian king." But their comment on no. 169 is hardly adequate; the inscription (from Mesogeia, Attica) reads:

Τόδ' Ἀρχίου στι σῆμα κάδελφης φίλης·  
Εύκοσμίδης δὲ τοῦτ' ἐποίησεν καλόν·  
στήλην ἐπ' αὐτῷ θήκε Φαιδιμο<s> σοφός.

This is more than "a plain report with the adjective at the end of each verse as the only ornament" (p. 157): the adjective bears all the weight of the affection between brother and sister, of father for children and children for father; the father was proud of the memorial he erected for his children; and the craftsman felt that he could boast of his skill; thus φίλης, καλόν, σοφός acquire significance. Similar to the boast of Phaedimus is that of two Argive craftsmen who stressed the long tradition which they carried on in their work:

Εὐτελίδας καὶ Χρυσόθεμις τάδε ἔργα τέλεσσαν  
Ἀργεῖοι τέχναν εἰδότες ἐκ προτέρων

(no. 154, derived from Pausanias, VI, 10, 5, probably *ca. 550*).<sup>4</sup>

The literary level of these inscriptions may not be high, but they preserved a rhetorical technique which—like many features of pre-classical literature—apparently found little favor during the fifth century and re-appeared only in the literature that followed the so-called "classical" period, usually after 300 B. C.

Among the contemporaries of Leonidas some instances of this final emphasis are to be found in Asclepiades. One poem, *Anth. Pal.*, V, 85, deserves greater renown, I fancy, as anticipating Marvell's *To his Coy Mistress*:

Φείδη παρθενίης; τί τὸ πλέον; οὐ γὰρ ἐς Ἄδην  
ἐλθούσ' ἐνρήσεις τὸν φιλέοντα, κόρη.  
ἐν ζωῖσι τὰ τερπνὰ τὰ Κύπριδος· ἐν δ' Ἀχέροντι  
ὅστεα καὶ σποδιή, παρθένε, κεισόμεθα.

<sup>4</sup> Many inscriptions, like Simonides 115 (Diehl) cited above p. 279, seem to have a well-chosen final word: cf. nos. 8, 28A, 43, 61, 64, 76, 79A, 95(a), 132.

Preserve you your virginity? but why?  
 When you reach the grave  
 You'll find none to embrace you there, good maid.  
 No, while life is ours  
 Make am'rous sport. We'll lie, coy maid, in death  
 But as dust and ash.

'Εν δ' Ἀχέροντι | ὁστέα καὶ σποδιή—this is natural enough; there is no life in Acheron. But note the grim irony of the last two words *παρθένε, κευσόμεθα*—the result both of their juxtaposition and of their relation to the poem as a whole: We shall lie together; but what can bones and ashes do? the answer lies in *παρθένε!*

The worms shall try  
 That long-preserved virginity,  
 And your quaint honour turn to dust,  
 And into ashes all my lust.

Somewhat similar in its final irony is *Anth. Pal.*, XII, 50. The unhappy lover, Asclepiades, is to drown his sorrows in drink. Twice, after the initial *πίνε*, we meet the resounding *πίνωμεν*. As for his night of love—*τὴν μακρὰν νυκτὸν ἀναπανσύμεθα*. This is, of course, Catullus' *Nox est perpetua una dormienda*; but to the disappointed lover its connotations would be more intense: *ἀναπάνεσθαι, restfulness*, was hardly what he had hoped the night would mean for him.

Sometimes emphatic summary without surprise or irony is what we find. *Anth. Pal.*, V, 44 bids us beware of those pirates, the street-walkers; for once you grapple with them—and the puns are there—you're sunk! And from *Anth. Pal.*, V, 169, with its final *ὑπὸ ἀμφοτέρων*, emphasized by a preceding *μία* we learn that the sweetest thing in life is love, if its joys be willingly shared by both.

The close of *Anth. Pal.*, V, 64 may perhaps have a slightly satirical tone. Asclepiades avers that nothing will prevent his pursuit of love; "Even you, O Zeus, were subservient to Love when, as gold, you entered through bronze doors (*διὰ χαλκείων χρυσὸς ἔδυς θαλάμων*)."<sup>1</sup> It may be that a juxtaposition of *χαλκείων* and *χρυσός* is sufficient to justify the phrasing; but, in view of the gay professional society of which Asclepiades writes, one wonders if the all-conquering gold which won access to the lady "bound in brass" was not more negotiable than the mythological shower of gold which tempted Danae.

Hardly a trace of the literary phenomenon I have been discussing is found in Asclepiades' associate, Posidippus. There is an occasional flick of the whip in the third member of the group, Hedylus, but too few of his poems are preserved to make any judgment satisfactory.

In contrast with this at least 25 of Leonidas' poems, besides those interpreted above, are marked by effective emphasis at the close of the epigram—altogether almost one third of the total preserved. A few of them may be referred to here:

*Anth. Pal.*, VII, 67: Diogenes enters Charon's boat with very little baggage; so it is with all—"you can't take it with you"; but Diogenes never had any more, anyhow, as the last words show—λείπω δ' οὐδὲν ὑπ' ἡελίῳ.

*Anth. Pal.*, VI, 226: The first three verses, with three forms of ὀλίγος, emphasize the poverty of Cliton's property; but, in spite of this, his life span was great—eighty years, as the last verse tells us.

*Anth. Pal.*, VII, 264: This poem begins with a wish for fair sailing for a departing seaman; the perils of bad weather are next suggested; and then v. 4 points out that the departing seafarer had attached his cables to the speaker's tomb—and τάφον is the final word. The implication is obvious.

*Anth. Pal.*, VII, 466: A bereaved mother asks that she be healed by death; ἵητὴρ ζωῆς ἐκ με κομισσάμενος—"become my healer by removing me from life."

*Anth. Pal.*, VII, 652: This poem describes the fate of a merchant who was lost at sea; since he was lost σὺν φόρτῳ, it is but fitting that the poem should end with his name Τελευταγόρην (*final marketing*).

*Anth. Pal.*, IX, 99: The last example I offer has a subject which naturally encouraged the use of a final "epigrammatic" point; for it is a kind of fable, such as we find in Aesop's collection. A goat stripped a young vine, which then assured the goat that it would gain nothing by such cruelty, since the vine would persist as wine, to triumph finally as a libation when the goat was sacrificed—and the final word is θυομένῳ.<sup>5</sup>

None of these poems has the sharpness or the biting quality so characteristic of Martial's verses; but they do form a group of

<sup>5</sup> For other examples, cf. *Anth. Pal.*, VI, 120, 298 (cf. 293), 300, 355; VII, 173, 273, 422, 472, 472b, 648, 654, 726; IX, 179; *Plan. App.* 206; Stobaeus, *Flor.*, IV, 52, 28.

short poems which end "in a witty or ingenious turn of thought," written long before the form became popularized by Martial and his contemporaries. If Leonidas is unique or almost unique as the author of such poems in the early Hellenistic period, his stature as a writer of epigrams is increased; if he is simply one of a group, he is symptomatic of a stage in the development of epigram that should be given a greater attention.<sup>6</sup>

CHRISTOPHER M. DAWSON.

YALE UNIVERSITY.

<sup>6</sup> Leonidas has suffered much from the severe criticisms of such scholars as Geffcken and Wilamowitz; more favorable is the attitude of P. Wuilleumier in his study of Leonidas in his *Tarente des origines à la conquête romaine*, pp. 633-656 (= *Bibliothèque des Ecoles Françaises d'Athènes et de Rome*, fasc. 148 [1939]). Wuilleumier points out, in connection with the Maronis epigram, that Leonidas may well have seen Tarentine graves with drinking cups adorning them (pp. 549 and 636); he also notes the humor of the Eubulus poem (p. 644), but goes no further along this line of investigation.

## FIVE MINOR ELUCIDATIONS IN SENECA'S MORAL EPISTLES.

33, 5. *Res geritur* seems to have escaped criticism for centuries; this is confirmatory of the view that Seneca's prose has always been sloppily read.<sup>1</sup> Hense (ed. 2) records, however, Madvig's dissatisfaction with *geritur* and his wish to replace it with *seritur*, "is arranged," "is put together."<sup>2</sup> The expression itself *res geritur* is a fine Roman phrase ("business is being transacted") but does not fit here without some sort of qualitative; all the emendatory proposals reflect this feeling. They are these:<sup>3</sup> (a) Summers' suggestion that *tota tractanda* be transferred from the end of the preceding sentence and attached to *res geritur* to form the new sentence beginning; (b) Axelson's <*continuando*> before *res geritur*; (c) Beltrami's <*re*> *res geritur*, which means, I suppose, "the business is actually going on"; (d) H. Georgii's moving of the *et* after *geritur* to follow *sua*, which has the effect of giving *res geritur* the complement *per lineamenta sua*; (e) my own idea, shared with Summers in another of his approaches, of a dropped adjective needed to modify *res*, such as *integra* or *magna* or *ingens* or *tanta*.

For the understanding of *res geritur* we must look forward to the parallel provided in the rest of the sentence; the problem is to make this clear by the mechanics of language. On fuller consideration I now suggest *res geritur* <*ita*> *ut*. At some point in the MS tradition the *-itur* was written as *-it* plus a conventional sign for the *-ur*, the whole resembling <*ita*> so much as to provide a good chance for an haplography. With <*ita*> out the *ut* was no longer clear, and was altered to *et*. The alteration to *et* looked plausible but was the beginning of a total misunderstanding of the passage. With the proposed emendation trans-

<sup>1</sup> Cf. H. W. Garrod, *C. Q.*, VIII, p. 272.

<sup>2</sup> *Epistulae Morales ad Lucilium* (ed. 2, Leipzig, 1914), p. 108, note. Brakman's *per partes igitur*, recorded in the same note, while quite unacceptable, points in the right direction.

<sup>3</sup> Successively: (a) *Select Letters of Seneca* (London, 1914), p. 33, note; (b) *Neue Senecastudien* (Lund, 1939), note to pp. 184-185; (c) *ad Lucilium Epistulae Morales* (Rome, 1937), p. 127, note; (d) *Philologus*, LXXXIV, pp. 84 ff.; (e) Summers, *ut supra*, and *California Series in Class. Phil.*, XII, No. 5, pp. 72-73.

late: "the business [of building up the Stoic philosophical exposition] is conducted just as a masterpiece is developed by the interweaving of line upon line, from which nothing can be subtracted without ruining [the whole]."

47, 8. After a long and detailed account of the highly specialized functions of the chief household slaves in a great establishment, which bring the master into rather intimate relations with these slaves, Seneca caustically adds: "In company with these he cannot bear to dine, and regards it as an impairment of his dignity to come to table with his slave." Then comes the familiar exclamation: *di melius!*

This, it appears to me, must in the context be regarded as the great man's indignant comment on the mere idea of such social degradation. Hence alter the period after *accedere* to a semicolon, and, enclosing *di melius!* in quotation marks, complete the sentence with it. The words following, it will now be seen, are not exclamatory through association with *di melius!* They stand on their own feet, and should, in my opinion, be printed as a question. When they are so printed and read, the full contrast is achieved with infinite neatness between the angry exclamation of the great lord and the quiet question which is the full comment on his sneer; it is addressed to the audience, the readers of the letter, both Lucilius and ultimately the general public. "How many of these people" (i. e., the class of former slaves whose nature has not changed with their legal betterment) "has the user of the *di melius!* expression for his masters—like Callistus, for example?"<sup>4</sup>

53, 9. Through paragraphs 9, 10, 11, 12 we have all the illustration necessary of that stylistic fault of unorganized, paratactic observations occurring in rapid succession, which gave Caligula grounds for his judgment that Seneca's literary manner was *harena sine calce*. The editor is likely in such places to be puzzled about the proper punctuation, and may indeed simply throw up his hands. There is, however, a certain amount of patterning likely to be found operating in a number of these

<sup>4</sup> For the sneer cf. paragraph 13, *nihil . . . turpius*, and for former slaves becoming masters, there are, after the specific Callistus story, some general remarks from paragraph 11 on with regard to possible changes of social status in this strange world of ours.

places, and of this I should like to submit a sample, as it appears to me, running from *exercet* to *iubet*. I believe that a careful study of the numerous *harena sine calce* passages might show much more *calx* through patterning than has been thought to exist.

Philosophy is a queen, actively engaged in her function (*exercet regnum*). How is this shown? By a positive statement (*dat tempus*), followed by its natural opposite (*non accipit*), by a negative statement (*non est res subsiciva*), followed by its positive form (*ordinaria est*), the whole proof being thus presented in a chiastic arrangement. We then seal the proof by a statement in general resembling that with which we began, viz.: *domina est, adest et iubet*. I retain the reading of all the MSS including Q; the *est, adest et iubet* is too obviously the favorite double cretic clausula to allow us to think of tampering with it.

My proposal for the punctuation of the passage is, therefore, as follows: *exercet philosophia regnum suum—dat tempus, non accipit; non est res subsiciva, ordinaria est—domina est, adest et iubet*, with the territory common to the opening and the concluding statement lying as a common parenthesis between them. The translation is: "Philosophy actively governs her kingdom—granting appointments for interviews, not seeking them; no spare time matter, but an affair of every day in due course—a great lady, always on the task, always issuing the orders." The opposite figure, by the way, that of Fortuna, will be found in *Dial.*, VI, 10, 6 *fin.*: *varia et libidinosa mancipiorumque suorum neglegens domina*.

The *res* in the parenthesis seems intrusive, interfering with the personal idea that otherwise dominates the sentence, but Seneca has, as not infrequently, failed to preserve uniformity in his presentation.

57, 6-9. "What difference does it make whether it is a watch-tower or a mountain that falls on a person? Yet you will find some who are more afraid of the collapse of a mountain, although each of the two collapses is equally deadly; fear fixes its eye not on what actually happens, but on what makes it happen. 7: Now you think that I am talking of Stoics who hold that the vital principle of a man who is completely crushed by a great weight, cannot endure and is forthwith dissipated, because it found no free exit."

It is objected that such was not the Stoic belief. Certainly not the official (if one may so put it) Stoic belief, but there may have been those who, while on the whole Stoic in creed, made reservations on particular points. We have to remember also that *de Stoicis* can mean "about Stoics" as well as "about the Stoicism." Seneca continues: "I do not do so," using a representative *facio* to save repeating *existimo* (not *dico*, as Summers, *op. cit.*, p. 242, thinks). "People who say that, are, in my opinion, wrong."

Section 8 is entirely taken up with the assertion that the soul, being composed of the very finest particles, can no more be trapped, and thus crushed to death, than fire can be put out or air struck or cleft by a blow. The soul is of finer substance than fire; yet the far-ranging fire of the lightning flash returns through the tiny aperture from which it issued. Thus the soul, finer far than fire in its composition, has a passage (*fuga*) through any and every material. As the proof positive has now been laid down that the soul, in virtue of its nature, can escape through all materials whatsoever, I should read the opening sentence of section 9, *itaque . . . esse*, not as a statement, but as a question, particularly appropriate as Seneca evidently wants to close the discussion, thus: "Consequently, in relation to the soul, do we have to discuss whether it can be immortal?"

Thereafter Seneca becomes hurried and compresses his argument. I agree with Summers (*op. cit.*, p. 67, note) that *propter quod non perit* is a gloss on *si superstes est corpori* which has been taken into the text. Translate: "Treat this at all events as certain: if the soul survives the body [as we have shown it does], it cannot be ground down to nothing in any way whatever, since immortality [that is, not-dying] is absolute, admitting of no reservation or exception, nor is there anything at all which is deadly to that which has eternal life."

When Seneca says that immortality is an absolute term, admitting of no reservation or exception, he is once more referring to what some people might say: "The soul of a man is immortal, excepting however in such a case as that in which he is crushed absolutely flat by a great slab." Seneca says that the reservation "excepting . . . slab" is inconsistent with the idea of non-liability-to-death (*immortalitas*).

77, 10 *fin.* This is a good example of the sentence treacher-

ously simple in appearance, but, on reflection, needing some exegesis, and seeming even to call, at times, for some textual amplification.

Text: *Saepe enim debemus mori nec volumus, morimur nec volumus.* That is the modern punctuation; Fickert has a colon. This indicates, I should judge, his feeling for the markedly antithetic character of the second sentence to the first, antithetic by way of extension of the idea, at least. "Often we ought to die, but we don't want to do so; we die [without any question of obligation involved], but we don't want to do so." The remarks following show that in the second case we are talking of death that comes, as we may say, in the course of nature. Consequently, the statement of the second sentence is, as already noted, antithetic to the statement of the first by sheer extension of the positive part of that statement. Thus the second sentence is of the type which suggests an *immo*, "[welches] bedarf in der Regel einen näher bestimmenden Zusatz, welcher dem Vorhergesagten entgegensteht."<sup>5</sup> The case is so excellently made out here that Summers<sup>6</sup> believes Seneca wrote <*immo*> *morimur*, comparing *morituris, immo morientibus* of 120, 17.

If *immo* is to be read, I would place it after *morimur* because (1) a good case for an haplography is thus set up, explaining the disappearance of the *immo* or *imo*; (2) a clausula of the cretic plus resolved trochee type (---|---) emerges, forming a parallel almost but not quite identical with *-mus mori nec volumus*. While before Livy's time *immo* always stood at the head of its sentence, from then on it is not uncommon to find it in second position, as the lexicons make clear.<sup>7</sup>

But the conclusion of this little elucidation should probably do nothing more than affirm the plausibility of *morimur immo* (or *immo morimur*), since so frequently in Latin, and especially in Seneca's Latin, antitheses are set up without formal notification of the fact. Perhaps this annoyed Caligula and confirmed him in his *harenam sine calce* fling.

WILLIAM HARDY ALEXANDER.

UNIVERSITY OF CALIFORNIA.

<sup>5</sup> Krebs-Schmalz, *Antibarbarus* (6th ed., Basel, 1886), I, p. 628.

<sup>6</sup> *Op. cit.*, p. 76, note.

<sup>7</sup> Krebs-Schmalz, *op. cit.*, I, pp. 628 and 629.

## THE MEANING OF *οὐπω*.

In a recently published paper<sup>1</sup> J. E. Fontenrose has referred to the commonly accepted view that *οὐπω* is sometimes used in Homer and elsewhere in the same sense as *οὐπως* "nowise" or *οὐποτε* "never," and has argued convincingly that this view is mistaken and that *οὐπω* always has the temporal sense "not yet." He discusses eleven passages in Homer in which at least one lexicographer or grammarian has declared *οὐπω* to be equivalent to *οὐπως*, and in most of them he shows that the sense "not yet" is either preferable or at least perfectly possible. For example in M 271

ἐπεὶ οὐπω πάντες ὁμοῖοι  
ἀνέρες ἐν πολέμῳ,

where all interpreters have given  $\pi\omega$  a modal force, he says rightly that its force is as usual temporal and that "there is an effect of purposeful understatement." The sense is "all men are not equal, at any rate so far, whatever may happen in the future." The speaker does not, of course, intend to suggest any real doubt about the persistence of this state of inequality, and it is true that the substance of his thought could be expressed by *οὐπως*; but merely to say that *οὐπω* is here equivalent to *οὐπως* would be to miss the tone of the expression, which is that of ironical understatement. Compare our expression "I have yet to learn."<sup>2</sup>

In μ 208 however

ὦ φίλοι, οὐ γάρ πώ τι κακῶν ἀδαιμονές εἰμεν,

Fontenrose's defence of the meaning "not yet" for *οὐπω* seems to me less convincing. He takes the sense to be "we are not yet without experience of dangers, but the danger to come is no worse than those we have already endured," and in support of the temporal sense of  $\pi\omega$  cites the Virgilian imitation (*Aen.*, I,

<sup>1</sup> "On the Particle ΠΩ in Homer," *A. J. P.*, LXII (1941), pp. 65-79.

<sup>2</sup> The result of this formal avoidance of an absolute statement (which would include the unknowable future), of this apparent limitation to the known past and present, is to make the statement, within these limits, all the more confident and categorical. Thus it is true that in this sense *οὐπω* may amount to a strong negative.

198) : *O socii, neque enim ignari sumus ante malorum.* Now it is clear that  $\pi\omega$  cannot have quite the same sense as *ante* "in time past," and when Fontenrose himself explains that whereas *ante* covers past dangers only, *οὐπω* "suggests both the coming danger and the past dangers," he shows himself conscious of some reference to the future in *οὐπω*. But what sort of reference is it? I take it that the essential force of  $\pi\omega$  in *οὐπω* is to place a time limit on the action of the negative, and that it implies the possibility of a reversal of the situation at some point subsequent to the time of the verb that is negated; for example, if that verb is in the present tense, *οὐπω* indicates that something is not true now but will or may become true in the future.<sup>3</sup> In this passage, however, such a reversal is logically excluded by the form of words; it will not make sense to say "we are not yet inexperienced (but at some future time we may become so)," since experience once gained cannot be cancelled.

In Sophocles, *O. T.* 105

ἔξοιδ' ἀκούων· οὐ γὰρ εἰσεῖδόν γέ  $\pi\omega$ ,

the possibility implied in  $\pi\omega$  of seeing Laius in the future is not excluded by the form of words, but seems to be ruled out by the circumstances, since Oedipus must have known when he ascended the throne of Thebes and married Iocasta that Laius, his predecessor and the former husband of Iocasta, was dead.<sup>4</sup> Fontenrose attempts to justify the ordinary temporal sense of *οὐπω* by supposing that Oedipus is looking back to a time when he did not yet know that Laius was dead, and that he is saying that he had not seen him up to that time. To quote his words, "The chorus have reminded Oedipus of his coming to Thebes (35-39); Creon has just mentioned his succeeding Laius; and that time is never far from his thoughts throughout the play." But if "that time" means the time of his accession to the throne he must have known then that his predecessor was dead. There may have been a time immediately after Oedipus' arrival in

<sup>3</sup> The whole sequence can, of course, be shifted back into the past or forward into the future.

<sup>4</sup> It is true that in 103 Creon says  $\eta\tau\ \etaμίν, \ \ddot{\alpha}ναξ, \ Λάιός \ ποθ' \ \etaγεμών,$  as though Oedipus might never have heard of him, but this is really only a formal reminder, as Oedipus' reply shows. Possibly Sophocles is taking the opportunity to make sure that the audience are clear on this point.

Thebes when he was not yet aware of Laius' death, and perhaps had not even heard of him; and to look back so far would be to place himself at a point when the reversal properly implied by *οὐπώ* still seemed possible. But when Creon now speaks of Laius, is it likely that Oedipus would look back in thought any earlier than to the time when he first heard of Laius from the Thebans, and presumably heard that he was dead?<sup>5</sup> In any case it is, I think, more natural to take the sense of the line to be, "Yes, I know about Laius, but only by hearsay, for I never saw him."

Thus when *πω* is used elsewhere a reversal in the future is always either seriously envisaged or at any rate theoretically possible, but in Sophocles, *O. T.* 105 and Homer, μ 208 the possibility of such a reversal is ruled out by the circumstances or by the form of words. Fontenrose has shown that we cannot meet these difficulties by saying that μ 208 is an example of a Homeric use of *πω* as equivalent to *πως*, and it follows that we cannot suppose, with A. C. Pearson,<sup>6</sup> that in *O. T.* 105 Sophocles is imitating this Homeric use of *πω*. If *πω* is the true reading in either or both of these passages, it can only be explained, I think, on the assumption that there are three stages, logical not chronological, in the development of the meaning of *οὐπώ*: the first when a reversal in the future is seriously envisaged; this might be described as the standard meaning, and *οὐπώ* then corresponds closely to our "not yet"; the second, when the reversal in the future is not actually ruled out, but is not seriously envisaged, and the temporal limiting force of *πω* is used to impart a flavour of caution, real or ironically assumed, to the negative; owing to Greek love of irony and understatement this use too is quite common;<sup>7</sup> the third, when *οὐπώ* is used to impart this flavour of caution even in contexts where the future reversal which it should imply is impossible, and the temporal limiting force from

<sup>5</sup> Cf. *O. T.* 736-7.

<sup>6</sup> "Sophoclea," *C. Q.*, XXIII (1929), p. 91.

<sup>7</sup> Good examples in prose writers are Lysias, X, 30: *οὐ γάρ πω ἥδη ὅτι ὑμεῖς τὸν μὲν λδόντας τιμωρεῖσθε, τοῖς δ' ἀποβαλοῦσι* (*sc. τοὺς ἀσπίδας*) *συγγνώμην ἔχετε*, "I had yet to learn . . ." Xenophon, *Mem.*, III, 14, 2: *ἔσθιοντι μὲν γὰρ δὴ πάντες ἐπὶ τῷ σίτῳ ὅψον, ὅταν παρῇ· ἀλλ' οὐκ οἷμα πω ἐπὶ τούτῳ γε ὁφοφάγοι καλοῦνται*, "We don't so far call them. . . ." Herodotus, IV, 97, 4: *οὐ γὰρ ἔδεισά κω . . .*, "I'm not yet reduced to being afraid of Scythians."

which the flavour of caution was derived is no longer valid. Such a development as this third stage does not seem to me to be in itself inconceivable, but if it did in fact exist we might expect to find more examples of it.

If there are only two examples of this difficulty, it seems reasonable to suspect the text, and in both passages *πον*, often confused with *πω*, gives good sense. In *O. T.* 105 *εἰσεῖδόν γέ πον* is the reading of some late MSS and was favoured by Doederlein.<sup>8</sup> Lewis Campbell objects that *πον* "would imply uncertainty, and would therefore weakly express the security of Oedipus." But the words "I certainly never saw him" (Campbell's translation, assuming *οὐπω* = *οὐπως*) are unnecessarily emphatic, and Oedipus' feeling of security at this stage is as well or better exhibited by the complete indifference and unconcern of *πον*: "Yes, I have heard about him, though I never saw him, that I know of."<sup>9</sup> In Homer, μ 208 we should probably read *πον* for *πω*. In later Greek *οὐ πού τι* is a frequent combination, with the sense "surely not," generally in questions but also in statements,<sup>10</sup> and in Homer, A 124 *οὐδέ τι πον* is used in a very similar sense.<sup>11</sup> In our passage *πον* would thus give the satisfactory sense "for surely we are not without experience of hardship."<sup>12</sup>

Finally there are four passages from Tragedy which should perhaps be added to those cited or referred to by Fontenrose at

<sup>8</sup> *Reden und Aufsätze* (Erlangen, 1843-7), II, pp. 261-3. Fontenrose (from whom I take this reference) agrees that *πον* gives good sense here, though he prefers to keep *πω*. It may be noted that the collocation *γάρ, γε* and *πον* is quite likely. Denniston, *G. P.*, pp. 493-4, notes that Plato shows a fondness for the juxtaposition of *γάρ* and *γε* with *πον*, and cites *γε πον* from Sophocles and Aristophanes.

<sup>9</sup> Hartung proposed *εἰσεῖδόν γ' ἐγώ*, but the pronoun is rather too emphatic and tends to weaken the antithesis of *ἀκούων* and *εἰσεῖδον*. A. Y. Campbell, however, points out that *ἀκούων* refers to the fact stated in 104 and that a personal object is strictly needed for *εἰσεῖδον*, and accordingly suggests *εἰσεῖδόν σφ' ἐγώ*. Hartung's *ἐγώ* comes in rather more naturally in this juxtaposition with *σφε*: "he and I never met."

<sup>10</sup> Denniston, *op. cit.*, p. 492.

<sup>11</sup> Here, however, *πον* may be taken in its local sense "anywhere"; in P 446 *οὐ . . . τι πον* means "not anything anywhere."

<sup>12</sup> The corruption to *πω* is the more likely, since *οὐ γάρ πω*, in its proper temporal sense, is frequent at the beginning of a line, e.g. μ 186, κ 174.

the end of his article, since in them the temporal force of *οὐπω* has been or (on the current assumptions) might well be called in question.

Euripides, *Ion* 546:

*Iω. ἥλθες ἐς νόθον τι λέκτρον; Ξο. μωρίᾳ γε τοῦ νέου*

*Ιω. πρὶν κόρην λαβεῖν Ἐρεχθέως; Ξο. οὐ γὰρ ὕστερόν γέ πω.*

Here “not yet” is an instance of understatement, perhaps jocular in tone,<sup>13</sup> and means in effect “not at all,” though the implied possibility of reversal in the future is not ruled out.

*Ibid.* 1277-8:

*καὶ γὰρ εἰ τὸ σῶμά μοι  
ἀπεστιν αὐτῆς, τοῦνομ' οὐκ ἀπεστί πω.*

Here *οὐπω* is indignantly ironical:<sup>14</sup> “I have not yet reached the stage when . . .”

Sophocles, *Tr.* 1057-61:

*κοὺ ταῦτα λόγχη πεδίας, οὐθ' ὁ γηγενῆς  
στρατὸς Γιγάντων οὔτε θήρειος βία  
. . . . .  
ἔδρασέ πω.*

We should not perhaps use the word “yet” in this context, and Jebb’s translation “Not the warrior . . . hath ever done unto me thus” is adequate; but it is easy to see how in the Greek the characteristic limiting force of *πω* is operating here. The dying Heracles looks back to the time before he put on the fatal robe, and says that until he was vanquished by a woman’s hand no man or beast had so far been able to subdue him (whatever the future might have held).

Aeschylus, *Th.* 664-9:

*ἄλλῳ οὔτε νν φυγόντα μητρόθεν σκότον,  
οὐτ' ἐν τροφαῖσιν, οὐτ' ἐφηβήσαντά πω,*

<sup>13</sup> A. S. Owen in his note *ad loc.* suggests that it is “somewhat colloquial”; this sort of irony is certainly common in colloquial expressions, and a colloquialism is quite in keeping with the tone of this slightly comic scene from the *Ion*; on the other hand, I know of no good parallel in Comedy or prose dialogue or other sources for colloquial usage.

<sup>14</sup> Cf. Euripides, *Med.* 365 and Sophocles, *El.* 403, cited by Fontenrose.

οὗτ' ἐν γενείου ξυλλογῇ τριχώματος,  
 Δίκη προσεῖδε καὶ κατηξώσατο.  
 οὐδ' ἐν πατρώας μὴν χθονὸς κακονχίᾳ  
 οἵμαι νν αὐτῷ νῦν παραστατεῖν πέλας.

Tucker's note (on his 652) reads "πω (which belongs to the following verbs) can hardly be regarded as more than a metrical convenience."<sup>15</sup> There is, however, some point in πω, which has its usual temporal force. Eteocles is enumerating the various stages in the growth of Polyneices and says that at the third stage (έφηβήσαντα) Δίκη had not yet looked upon him. πω takes us back in thought to the time when it was still possible that she might do so in the future, and thus adds a touch of poignancy to the following lines in which we hear that this hope was in fact never fulfilled.

Thus none of these passages from Tragedy gives any ground for supposing that οὐπω ever has any force other than its usual temporal sense. The only passages that in my view present any difficulty are Homer, μ 208 and Sophocles, *O. T.* 105, and both can easily and satisfactorily be emended.

P. T. STEVENS.

UNIVERSITY OF LIVERPOOL.

#### HADRIAN'S PRECEDENT, THE ALLEGED INITIATION OF PHILIP II.

*Post haec per Asiam et insulas ad Achaiam (sc. Hadrianus) navigavit et Eleusinia sacra exemplo Herculis Philippique suscepit, multa in Athenienses contulit et pro agonotheta resedit.*

So, approximately, the passage *Vita Hadriani*, XIII, 1 appears in all the manuscripts and in the editions of Peter, Magie, and Hohl. It has been assumed that the words *exemplo Herculis Philippique* referred to the initiation of (Heracles and) Philip II of Macedon,<sup>1</sup> although there is no other reference to the

<sup>15</sup> He cites as parallel the use of πω in Euripides, *Ion* 1277 and Sophocles, *Tr.* 1061.

<sup>1</sup> See, for example, D. Magie, *The Scriptores Historiae Augustae*, I (London-New York, Loeb Classics, 1921), p. 40, n. 1; P. Graindor, *Athènes sous Hadrien* (Cairo, 1934), pp. 8, 123, and 124.

initiation of Philip II, the arch-enemy of Athens. If true, the reference to Philip II's initiation is an extraordinary piece of information, and there is no apparent reason why such a precedent should have been invented either by the Athenians, or by Hadrian, or by his biographer. The possibility that the reference is an invention of a propagandist in the fourth century after Christ may, in my opinion, be ruled out entirely. The reference to Philip is correct, or the information reflects a misunderstanding.

There is a real problem here which should not be ignored. If the information and text are correct, an interesting tradition concerning the policy of fourth-century Athens circulated under the Roman Empire, and a curious light is thrown on the aspirations of Hadrian. O. Th. Schulz,<sup>2</sup> who accepted the reference as correct, asked himself what the source could have been, and he decided that the source was Hadrian's autobiography. Kornemann<sup>3</sup> too attributed the information to the autobiography and accepted it as correct. Thereupon he faced the problem without flinching and wrote as follows: "Interessant ist, wie sich Hadrian in Griechenland neben der grossen Gestalt der Vorzeit (Hercules) auf Philipp von Makedonien beruft. Wie unter den Römern nicht Caesar, sondern Augustus, so war offenbar unter den Männern des hellenischen Kulturreises nicht Alexander sondern Philipp sein Vorbild. Wenn man bedenkt, welche Verehrung Alexander seit Caesar bei den römischen Imperatoren genossen hat, wird einem auch von hier aus wieder klar, wie Hadrian durchaus eigene Wege gewandelt ist."

Also Wilhelm Weber<sup>4</sup> approved the text as referring to Philip II, who "as a descendant of Heracles had had himself initiated in Eleusis like Hadrian, he too a stranger." For Weber the reference to Philip II is a piece of learning which puts us on the track of a lost historian. "Die Gelehrsamkeit . . . stammt nicht von dem letzten Bearbeiter der vita, eine ganz hervorragende Kenntnis der fingierten dynastischen Genealogien ist dazu unentbehrlich. Der das geschrieben hat, ist sich bewusst ge-

<sup>2</sup> *Leben des Kaisers Hadrian* (Leipzig, 1904), p. 61, n. 155.

<sup>3</sup> E. Kornemann, *Kaiser Hadrian und der letzte grosse Historiker von Rom* (Leipzig, 1905), p. 51.

<sup>4</sup> *Untersuchungen zur Geschichte des Kaisers Hadrianus* (Leipzig, 1907), pp. 168-174.

wesen, dass er zu einer reinen Überlieferung ein fremdes Element fügt, dass die Konstruktion der Ahnenreihe des Weltkönigs [i. e. Hadrian] des Philipp dringend bedarf." After reminding the reader significantly that the word *exemplo* occurs in two other passages of the *Vita*, Weber continues, "Es ist ein Historiker, dessen Verständnis für die wenigen Ideen der an die Vergangenheit sich klammernden Zeit grösser war als unsere reale Kenntnis, der zugleich die Hofliteratur gekannt und ihre Anschaungen in richtiger Weise verarbeitet hat." To what degree this lost historian drew upon Hadrian's autobiography a cautious scholar, in Weber's opinion, would not venture to say.

The other path had already been taken by the distinguished French student of the Mysteries, Paul Foucart. In the *Revue de philologie*, XVII (1893), p. 199 he proposed a correction of the traditional text *Philippi* into *Aesculapii* on the grounds that no ancient author mentions a sojourn of the Macedonian king at Athens, whereas the metrical inscription now published as *I. G.*, II<sup>2</sup>, 3575 mentions Castor and Pollux, Asclepius and Heracles as the mythical *exempla* of initiated foreigners. The proposal to emend has, however, been poorly received; in fact neither Kornemann nor Weber even mention it. Graindor<sup>5</sup> knew but rejected Foucart's emendation on the grounds that the corruption *Philippi* out of *Aesculapii* was improbable, and he accepted Weber's explanation that Philip II qualified as a descendant of Heracles.

It seems to me absolutely incredible that the signal honor of an initiation into the Eleusinian Mysteries would have been bestowed by the Athenians on the hated victor of Chaeronea. Therefore I too am forced to presume that the manuscripts contain a corruption. Foucart proposed an emendation paleographically unacceptable, but he was on the right track. Once it is suggested it becomes, I believe, obvious that the source (rather than the original text of the *Vita* itself) must have referred to Philopappus.<sup>6</sup> In the first place *Philippus* is an easy misreading for the rare and, later, unfamiliar name *Philopappus*. In the second place, Hadrian did, in other ways, really follow the lead of Philopappus.

<sup>5</sup> *Athènes sous Hadrien*, p. 8, n. 2.

<sup>6</sup> The information probably did come from the emperor's autobi-

The dispossessed prince Philopappus, son and co-regent of king Epiphanes of Commagene who lost his throne in 72 A. D., settled at Athens and died there between 114 and 116 A. D. The adoption of Athens by Philopappus and of Philopappus by Athens set a precedent for Hadrian. Philopappus had been enrolled in the deme Besa.<sup>7</sup> When Hadrian became an Athenian citizen he too chose Besa.<sup>8</sup> Philopappus consented to serve as archon, so did Hadrian. Philopappus served as agonothete;<sup>9</sup> so did Hadrian. Philopappus conferred upon Athens many generous benefactions; so did Hadrian. It can be assumed that Philopappus like many less prominent benefactors received the privilege of initiation into the Eleusinian Mysteries, so that Hadrian could point to the ancient precedent of the initiation of a stranger Heracles and also to a worthy, recent precedent provided by Philopappus.<sup>9a</sup>

It must be remembered that Philopappus was not just a rich man. He was a symbol. At his imposing memorial on the hill of the Muses, his statue stood between those of Antiochus IV of Commagene and of Seleucus Nicator. The dynasty had received the kingdom through Antiochus IV in 38 A. D. The statue of Seleucus Nicator, for us more significant, marked Philopappus as the representative of the glorious Hellenistic tradition.<sup>10</sup>

The position of the emperor has been well described as "the combination in one person of *princeps*, *imperator*, *basileus*."<sup>10a</sup> In Trajan, *imperator* and *optimus princeps*, the *basileus* was still a feeble note. Domitian had been more a tyrant than a

ography. The corruption *Philippi*, on which the manuscripts agree, may well have been launched by the author of the *Vita Hadriani*.

<sup>7</sup> *I. G.*, II<sup>2</sup>, 3451 and 3112.

<sup>8</sup> *I. G.*, II<sup>2</sup>, 1764.

<sup>9</sup> *I. G.*, II<sup>2</sup>, 3112.

<sup>9a</sup> It is well known that everything taken from the *historia fabularis* rated as history and that Cicero, at least in his later years, liked to pair *exempla*, one from ancient history and one from modern history, in that order (cf. H. Schoenberger, *Beispiele aus der Geschichte, ein rhetorisches Kunstmittel in Ciceros Reden* [Diss., Augsburg, 1910], pp. 10 and 31). See also K. Alewell, *Über das rhetorische Παράδειγμα. Theorie, Beispielsammlungen, Verwendung in der römischen Literatur der Kaiserzeit* (Diss., Kiel, 1913).

<sup>10</sup> P. Graindor, *Athènes de Tibère à Trajan* (Cairo, 1931), pp. 166-169 and 200-202. *I. G.*, II<sup>2</sup>, 3451 for the inscriptions.

<sup>10a</sup> R. Syme, *J. R. S.*, 1946, p. 158.

true king. Hadrian, on the other hand, wished to appear vividly as a genuine *basileus*, not a tyrant nor a ληστὴς τῶν Ἑλλήνων but a *basileus euergetes* in the best Greek tradition. In the Athens of his time it was the tradition of which Philopappus, descended from the Seleucids, was the living or remembered representative.<sup>11</sup>

JAMES H. OLIVER.

THE JOHNS HOPKINS UNIVERSITY.

---

### OBSERVATIONS ON THUCYDIDES.

I, 74, 1. Τοιούτου μέντοι ἔνυμβάντος τούτου καὶ σαφῶς δηλωθέντος ὅτι ἐν ταῖς ναυσὶ τῶν Ἑλλήνων τὰ πράγματα ἐγένετο, τρία τὰ ὀφελιμώτατα ἐσ αὐτὸ παρεσχόμεθα, ἀριθμὸν τε νεῶν πλείστον καὶ ἄνδρα στρατηγὸν ἔνυνετώτατον καὶ προθυμίαν ἀοκνοτάτην· ναῦς μέν γε ἐσ τὰς τετρακοσίας ὀλίγῳ ἐλάσσους τῶν δύο μοιρῶν, κ. τ. λ.

An examination of the passage quoted above, especially of the latter part which has been examined up to the present by many scholars,<sup>1</sup> gives rise to three questions: First, is reference made here to the whole of the Greek fleet (*ἐσ τὰς τετρακοσίας*), a part of which had been furnished by the Athenians? Second, does the phrase *ἐσ τὰς τετρακοσίας* mean "to the four hundred" or "nearly four hundred"? Third, how should the phrase *τῶν δύο μοιρῶν* be explained? Moreover, can the arithmetical figures which arise after the interpretation of this passage be made to

<sup>11</sup> For reference to Hadrian himself as a βασιλεὺς εὐεργέτης see above all the Thyatirene decree, *Hesperia*, X (1941), p. 366, especially lines 14-15. For the Hellenic ideal, the βασιλεὺς εὐεργέτης, see E. Skard, "Zwei religiös-politische Begriffe EUERGETES-CONCORDIA," *Avhandlinger utgitt av Det Norske Videnskaps-Akadem i Oslo*, 1931, Hist.-Filos. Kl., No. 2. For Alexandria see A. C. Levi, "Hadrian as King of Egypt," *Numismatic Chronicle*, 1948, pp. 30-38.

<sup>1</sup> Selected bibliography: T. Arnold, *Thucydides, with notes, etc.* (2nd ed., Oxford, 1840), *ad loc.*; A. F. Didot, *Thucydide, avec des observations, etc.* (Paris, 1833), *ad loc.*; A. W. Gomme, *A Historical Commentary on Thucydides, Introduction and Commentary on Book I* (Oxford, 1945), *ad loc.*; G. Grote, *History of Greece*, V (1856), p. 111, note 3; J. J. Owen, *Thucydides, with notes* (New York, 1858), *ad loc.*; E. F. Poppo, *Thucydidis de bello, etc.* (Lipsiae, 1866), *ad loc.*; C. Thirlwall, *The Cabinet Cyclopaedia*, II (1836), Appendix IV, p. 397.

agree with the figures found in other ancient sources and how can this agreement be made?

A close study of these three questions reveals the following:

On the first question Gomme (*op. cit.*, note 1), who first posed it, observes: "But what is really curious is the use of the phrase *ἐς τὰς τετρακοσίας* or *τριακοσίας* with *ναῦς* (*παρεσχόμεθα*), to mean not the number of the Athenian contingent but the whole of which the Athenian contingent was a part. This seems impossible (cf. e. g. 100. i); and as we should probably read a number in agreement with Herodotus and the tradition, we should emend to *ἐς τὰς διακοσίας ὀλίγῳ ἐλάσσους* (*οῦσας*), or, since this omits all mention of the whole of which the two-thirds were a part, perhaps more simply, *πρὸς τὰς τριακοσίας* or *τετρακοσίας*."

However, the first of these two emendations, with its alternative reading as proposed by Gomme, has not only an inherent disadvantage mentioned by Gomme himself, but, as I will show later, it is not necessary to read a number in agreement with Herodotus and the tradition; although that can be done also, but not according to the approach of Gomme and others. Also the second emendation, after which the preposition *πρὸς* renders the meaning of the phrase doubtful, is not justified by any particular need. Indeed there is something curious in the use of the phrase *ἐς τὰς τετρακοσίας*. However, this is not unexplainable since the phrase *ἐς αὐτὸ παρεσχόμεθα* occurs earlier in the passage, and the ambassadors, in analyzing and interpreting that phrase beginning with *ἐς*, had to use a parallel form of expression. Consequently the use of the preposition *ἐς* (*ἐς τὰς τετρακοσίας*) is due to a previous *ἐς* (*ἐς αὐτὸ παρεσχόμεθα*) and the full meaning then is: *ναῦς μέν γε παρεσχόμεθα ἐς τὰς τετρακοσίας τῶν Ἑλλήνων ὀλίγῳ ἐλάσσους τῶν δύο μοιρῶν, κ. τ. λ.*

On the second question I agree with Owen (*op. cit.*) who, as far as I know, was the first to support the view that *ἐς τὰς τετρακοσίας* means "to the four hundred," with the addition of the views on *ἐς* I have expressed above.<sup>2</sup>

For the third question which now remains to be discussed, three principal opinions have been proposed. The first arises from the Scholiast who writes: *δύο μοιρῶν, ἀντὶ τοῦ τὸ δίμοιρον,*

<sup>2</sup> Cf. Thucydides, I, 100, 1: *τὰς πάσας ἐς διακοσίας*, where the absence of an article gives the meaning of "about two hundred."

ὅ ἔστι διακόσιαι ἑξήκοντα· ἵσαν γὰρ αἱ πᾶσαι τετρακόσιαι, κατὰ δὲ ἄλλους τριακόσιαι ὅγδοηκοντα. ταῦτα οὖν λέγει ὅτι αὐτὸὶ ἡμεῖς τὸ δίμοιρον δεδώκαμεν. According to this the number 400 is understood to be equivalent fractionally to  $\frac{3}{3}$  of which the Athenians furnished somewhat less than  $\frac{2}{3}$ .

According to the second opinion which was proposed by Poppe (*op. cit.*), if the number of the Greek fleet, which was 378 triremes (Herodotus, VIII, 48) is to be expressed by a round number, it can be expressed more closely by 400 than 300. But, because it would be an honor for the Greeks to claim that they gained a victory by fewer ships than they really had, Aeschylus (*Pers.*, 338), Demosthenes (*de Cor.*, 238; *de Symmor.*, 29), and Nepos (*Them.*, 3) give the number of ships as 300. Thucydides, therefore, must have done the same also in the passage where generally the reading is *τετρακοσίας*. Hence it is sure that, what is said is that the Athenians furnished somewhat less than two parts, two-thirds, that is 200 ships, as we know from Herodotus (VIII, 61), Demosthenes (*de Cor.*, 238), Diodorus (XV, 78), and Nepos (*Them.*, 3). Since, therefore, it is evident from other passages also, except Thucydides (I, 10), that here are meant two parts out of three, *τριακοσίας* in Thucydides is in complete agreement with Isocrates (*Paneg.*, 107).<sup>3</sup>

According to the third opinion which was proposed by Didot (*op. cit.*) the information given in the latter part of Thucydides' passage should be interpreted analogous to the information given in another passage of Thucydides (I, 10, 2: *καίτοι Πελοποννήσου τῶν πέντε τὰς δύο μοίρας νέμονται*), where the relation of the part to the whole is clear. Hence, *τετρακοσίας* indicates a division of the whole into four parts of which the Athenians furnished

<sup>3</sup> Thirlwall (*op. cit.*) believes that "Thucydides meant to state the true numbers, in which, if we read *τριακοσίας* for *τετρακοσίας*, he would have followed Aeschylus instead of Herodotus, whom indeed it is possible he had not read."

Arnold (*op. cit.*), agreeing that *τῶν δύο μοιρῶν* means two parts out of three, says that "it would seem that it was a favourite boast of the Athenians, though not a true one, that two thirds of the fleet which fought at Salamis were furnished by Athens." According to him the exaggeration could be achieved by two ways: by overrating the number of the Athenian fleet if the reading is *τετρακοσίας* ( $\frac{2}{3}$  of 400) or by diminishing the amount of the whole combined fleet to *τριακοσίας* ( $\frac{2}{3}$  of 300).

somewhat less than two parts, i. e. less than 200 ships. Thus Thucydides agrees with Herodotus (VIII, 44) and all those writers according to whom the Athenian contingent consisted of 180 ships.<sup>4</sup>

As is evident, the first of the opinions mentioned above exaggerates the number of the Athenian fleet to a number which is found in no ancient sources. The second opinion is based on a few instances of rhetorical exaggeration in order to create by an emendation in Thucydides' passage a similar exaggeration. The third opinion is based principally on the interpretation of the passage under study which corresponds to other passages. However, these three opinions have one characteristic in common, that at one time they agree with the 380 (Herodotus) or 300 (Aeschylus, Demosthenes, Nepos) ships of the Greek allies and at another with the 180 ships of the Athenians at Salamis.<sup>5</sup>

In my opinion, creating a rhetorical exaggeration in this case is unjustified. The desire of the orator to employ hyperbole is expressed and realized when his listeners are willing to accept with pleasure what he has to say and to experience a certain amount of pride. An orator may employ exaggeration also when it serves his particular purpose, providing it is not recognized as such by his listeners because they do not know the truth. If Demosthenes and other Attic orators<sup>6</sup> employed exaggeration in their addresses to the Athenians, they did so because exaggeration gave the Athenians reason to feel prouder because of their history, while at the same time, if a listener was aware that exaggeration was intended, he would not have objected to it. The case, however, of the Athenian ambassadors who spoke

<sup>4</sup> Grote (*op. cit.*) in supporting Didot's opinion says that "wherever such harmony (between Herodotus and Thucydides) can be secured by an admissible construction of existing words, it is an unquestionable advantage, and ought to count as a reason in the case, if there be a doubt between two admissible constructions." Furthermore Grote disagrees with those who would alter *τριακοσίων* into *τετρακοσίων* in Demosthenes (*de Cor.*, 238), saying that such emendations appear to him inadmissible in principle, because we are not to force different witnesses into harmony by retouching their statements.

<sup>5</sup> Owen (*op. cit.*), after an examination of what commentators previous to him had suggested, concludes that "there have been almost as many conjectures as commentators, but none of them in all respect satisfactory."

<sup>6</sup> See Isocrates, *Paneg.*, 90, 93 ff., 97; Lysias, *Epit.*, 27.

before the Spartans is entirely different. And actually the use of exaggeration which arises from the interpretation of the Scholiast or from the reading of *τριακοσίας* would have created among the Spartans an attitude of general distrust of the words of the ambassadors. Hence, neither the interpretation of δύο μοιρῶν by the 2/3 can be supported, nor can a reading leading to an exaggeration be justified.

In the passages directly preceding the one under consideration, the Athenian ambassadors speak, of course, of the defeat of Xerxes at Salamis. But, since by the phrase *ἐν ταῖς ναυσὶ τῶν Ἑλλήνων τὰ πράγματα ἐγένετο* which follows these passages the ambassadors' thoughts concerning the value of the fleet widened, the arithmetical figures do not refer specifically to the battle at Salamis, but to the whole period of naval actions, i. e. from the time they sailed to Artemision to the battle at Salamis. Consequently the number 400 must refer to the whole number of ships furnished by the Greek allies during that period. Hence no instance occurs which permits the number 400 to correspond to the number 380 to which Herodotus raises the Greek ships at Salamis; nor can the number 180 mentioned by Herodotus also as the number of the Athenian ships at Salamis correspond to the number intended by the phrase *δλίγῳ ἀλάσσοντος τῶν δύο μοιρῶν* without creating or continuing an exaggeration.<sup>7</sup>

If after this discussion we should like to create a basis for correlating the numbers given by the ambassadors according to

<sup>7</sup> Herodotus himself reveals the exaggeration, because he says that the Athenians sailed to Artemision with 180 ships (VIII, 1, 14) but he speaks also of the losses which the Greek fleet suffered there (VIII, *passim*). After those losses one cannot believe that the Athenians had their entire fleet intact at Salamis also.

Herodotus also finally (VIII, 82) gives a sum of 380 ships, although the sum is not correct. On examining all his information concerning the number of the Greek fleet at that time (G. Papantoniou, *Προβλήματα περὶ τὴν συγγραφὴν τοῦ Ἡροδότου* ['Αθῆναι, 1949], pp. 84 ff.) I found that by increasing the number of the ships at Artemision (324) with the 45 captured Persian ships and 2 deserters and by subtracting 5 ships captured by the Persians, we have the number 366, i. e. the number which also is the real total of Herodotus' figures (VIII, 43-47). This led me to an explanation of how Herodotus calculated the number of Greek ships at Salamis, i. e. I suggested that though he says *πάρεξ τῶν πεντηκοντέρων* (VIII, 48) he carelessly added the 14 pentekontoroi to the 366 and thus reached the total of 380 ships.

Thucydides with the numbers as given by Herodotus, we should take, I think, the following course.

The number given by Herodotus (VIII, 48) is not in fact the number of the Greek ships at Salamis, but rather the total number of ships furnished by the Greek allies from the time they sailed to Artemision to the battle of Salamis. Thus, during all that time 366 ships (Herodotus, VIII, 43-47) had been furnished, 310 of which (Aeschylos, *Pers.*, 338) were at Salamis. The remaining 56 ( $366 - 310 = 56$ ) had been sunk, captured, or seriously damaged during the naval action at Artemision (Herodotus, VIII, 16, 17, 18) and therefore did not take part in the battle of Salamis. If to these 366 ships we add 14 pentekontoroi (Herodotus, VIII, 1-2, 46-48), two deserters, and an uncertain number of ships with which the Aeginetans were guarding their island (Herodotus, VIII, 46) we can easily reach a total of 400, a round number which, according to Thucydides, the Athenian ambassadors mentioned. In addition the same ambassadors mean by the phrase ὅλιγῳ ἐλάσσονς τῶν δύο μοιρῶν that of the 400 ships the Athenian contingent amounted to 180 ships ( $127 + 53$ ), those by which the Athenians set sail to Artemision, because no other ships were furnished later by them.

GEORGE A. PAPANTONIOU.

GREEK THEOLOGICAL INSTITUTE, BROOKLINE.

## ANNOUNCEMENT.

---

### PROSOPOGRAPHY OF THE LATER ROMAN EMPIRE

A committee has been set up under the auspices of the British Academy with the object of compiling a prosopography of the later Roman Empire (284-641 A.D.). Its object is to do for the later Empire what the Prosopographia Imperii Romani has done for the Principate, to provide the materials for the study of the governing class of the Empire. The majority of the entries will be persons holding official posts or rank together with their families, and the work will not include clerics except insofar as they come into the above categories.

The French Institute of Byzantine Studies are simultaneously launching a Christian prosopography covering roughly the same period (300-700 A.D.) which will include all persons, whether laymen or clerics, who play a part in the history of Christianity.

The two committees have agreed to cooperate in the collection of material, since though their aim is different, they both draw on the same sources. They cordially invite scholars interested in these projects to assist in whatever way they can.

There is a vast range of material to be worked through, including inscriptions and papyri, the Codes and the Councils and patristic and hagiographical literature, not only Latin and Greek, but also Syriac and Coptic. It would be convenient to the editors if those who wish to help would undertake responsibility for a definite author or other group of sources.

In order to establish the origins and connections of the entries and to make certain that no references to them are missed it will be necessary to collect many more names than will ultimately be published. After discussion it has been decided that it will be the only practical course to collect all references to personal names in literary sources. In dealing with inscriptions and papyri this would involve unnecessary labour and special instructions have been drafted. Will any persons prepared to help please communicate with either:

Professor A. H. M. JONES,  
Department of Ancient History,  
University of London,  
University College,  
Gower Street, W. C. 1

or

Professor H. I. MARROU,  
Université de Paris,  
Faculté des Lettres (Histoire Ancienne du  
Christianisme),  
Sorbonne, Paris.

## REVIEWS.

MARCHINUS H. A. L. H. VAN DER VALK. *Textual Criticism of the Odyssey*. Leiden, A. W. Sijthoff, 1949. Pp. 296. 14.50 fl.\*

The author was the minister of the Netherlands Reformed church in Wyngaarden, and during the German occupation devoted himself to the collection of material for this book. Its index (pp. 290-296) lists over 400 passages from the *Odyssey*, some 70 from the *Iliad*, and others from the scholia of both poems, from Hesiod, and from other Greek Authors. Another page (p. 8) is filled with the abbreviations used for modern works on Homer, and this represents, of course, only a fraction of the author's reading. Such energy and industry command attention and respect; and so I regret that I must frequently disagree with the conclusions to which they have led. Our basic outlooks are so different that a thorough discussion would involve the writing of another book—a restatement of views I have published,<sup>1</sup> and that I cannot hope to express in more convincing fashion. I can see nothing to be gained by this, and shall confine myself to a brief treatment of a few topics.

The author (p. 250, n. 3) ascribes both the *Odyssey* and the *Iliad* "to the same poet." He was an Ionian, named Homer, who (p. 76) lived "in all probability in the seventh century B. C." He is placed (pp. 231-238) after Hesiod; and after (so the author seems inclined to think) the Kyklos. He himself wrote his poems, not using (p. 68) the digamma, and divided them (pp. 264-266) each into twenty-four books. His work was performed practically in one jet, for all distinction between early and late passages is repudiated.

The author differs from some—and it is to his credit—in having a definite aim. He states it (p. 48): "We saw how the vulgate in many places, in spite of [would-be] improvements made by ancient scholars, has preserved the original text. It is our opinion therefore that in establishing the text we can go beyond the Alexandrians and can get back to the original text of the poet. Not the Alexandrian text but the original text of Homer must be our aim in the construction of the text, an aim, which, as we see, is attainable." One may not see eye-to-eye with the author, one may call the aim optimistic; but one should, in my opinion, be sympathetic with the effort to attain this goal—or something like it. Whether this can be done with the method employed by the author seems to me more than doubtful.

\* This review was prepared with assistance provided by a Minor Grant of the American Council of Learned Societies which is hereby gratefully acknowledged.

<sup>1</sup> *External Evidence for Interpolation in Homer* (Oxford, 1925); *The Athetized Lines of the Iliad* (Special Publication of the Linguistic Society of America [Baltimore, 1944]); review of von der Mühll's edition of the *Odyssey*, *A. J. P.*, LXIX (1948), pp. 210-213; "On Editing the Homeric Poems," *A. J. P.*, LXX (1949), pp. 367-75; an edition (in press) of the *Iliad*—a reconstruction of the text that circulated in Athens in the 6th century B. C.

First, however, a matter seemingly small, but in a way important. Ascribing the book division to the poet—the author is not the only man who does this—comes from confusing the poem with its format. The *Odyssey* is just as good a poem as it is, whether it is divided into books or not, whether it is bound in one volume or in two, whether it is covered with green or red. The aesthetic considerations adduced show merely something that did not need to be shown—that the man who made the division did not chop the text into twenty-four equal sections, but allowed his action to be determined by the joints found in the story. Whether his division is the best possible has been debated: Wilamowitz believes that it is not as good as the job done on the *Iliad*, while Leaf has shown where the latter could be improved. Good or bad, the division was imposed upon the tradition ca. 150 B. C. The papyrus evidence for that is clear. At the same time came the change in the content of the texts, first noted by Grenfell and Hunt, and the editorial regulation of paragogic *nu* at the verse ends. Taken together the three mark a crisis in the transmission of the poems. That is their importance.

The chief flaw in the author's method is, in my opinion, his undue exaltation of the vulgate (the MSS ranging from the 10th to the 16th century). Inerrancy is, of course, not claimed for it; indeed such a claim is expressly disavowed. Still by the side of the vulgate all other sources of information pale—the fragmentary texts that run back to the 3rd century B. C.; the readings of Alexandrian scholars recorded in the scholia; quotations, pre-Alexandrian or later; imitations in Greek poetry. The usual opinion is that among verbal variants offered in any of these ways we are free to choose that which seems intrinsically the most probable. An expression of such an opinion by Wilamowitz is quoted (p. 36) and brushed off: "This view is decidedly erroneous." In these variants the author sees conjectures. Sometimes—when they are "more or less tempting"—they get into the MSS to a greater or less extent; but still they are conjectures, and the conjectures can be "unmasked."

An example of this procedure is found (pp. 78-79) in his dealing with:

- ω 275 δῶκα δέ οἱ κρητῆρα πανάργυρον ἀνθεμόεντα
- 276 δώδεκα δ' ἀπλοῖδας χλαίνας τόσους δὲ τάπητας = Ω 230
- 277 τόσα δὲ φάρεα καλά, τόσους ἐπὶ τοῖσδε <sup>1</sup> χιτῶνας = Ω 231

The passage is full of haplographic traps, and MSS seem to have been caught in them one way or another. That is not unusual or important, and the author is not to be criticized for saying nothing about it. However, P 28 (3/4 p.) contains the passage, and the author comments: "In agreement with the formular style of Homer it may be expected that such lines are repeated. The pap. thought it incorrect that on the occasion of an ordinary visit equally valuable presents should be given as for the ransom of Hector and therefore omitted [line 277]." The papyrus actually omits only the words I have put in half brackets. That is evidently due to haplography—nothing more.

Another example is his treatment (pp. 268-269) of

- ι 530 δὸς μὴ Ὁδυσσῆα πτολιπόρθιον οἴκαδ' ικέσθαι — ι 504
- 531 [νιὸν Λαέρτεω, Ἰθάκῃ ἔνι οὐκὶ ἔχοντα.] = ι 505

"This line is only found in two mss. and in Macrobius 5.12.6 Modern critics agree that it is a spurious repetition of  $\iota$  505, as Polyphemus need not mention the name of Odysseus' father etc. to Poseidon, who is no doubt acquainted with these facts. In so reasoning one misunderstands archaic mentality. For in this place we have to deal with a curse and in this case it is of great importance that the person affected by the curse, is named as accurately as possible, that the avenging god might strike his victim with unerring aim. Therefore Polyphemus repeats [ $\iota$  530-1] as accurately as possible the words spoken by Odysseus to reveal himself ( $\iota$  504-5); on purpose he does not omit any particular. In § 27 we saw that the Alexandrians sometimes removed original lines from the text. Now  $\iota$  531 could not but seem a spurious repetition to the Alexandrians, who did not pay heed to archaic mentality. Therefore we can understand that they not only atheized it but removed it from the text as well (cf. similar examples § 27). Their faulty elimination met with success, as it was taken over by nearly all the mss. This too can be understood, when we compare § 17. There we saw how the emendations of the Alexandrians were taken over by the vulgate, whenever they seemed obvious."

The author has not heeded three facts. (1) The line is not in a text written in the 3d century B. C. (P. Jouguet, *Journ. de l'Égypte anc.*, I [1925], pp. 1 ff., cited elsewhere). (2) The line is not quoted by Macrobius. It has been added to his text in modern times to make him agree with Wolf. (3) Both MSS that contain the line are of the 16th century. In view of these facts I conclude that in, or slightly before, the 16th century someone either thoughtlessly repeated  $\iota$  505, or thought that he knew how to curse, and that he could improve the text with little effort.

Similar (but no more cogent) arguments are put forward in favor of  $\sigma$  184a,  $\psi$  127-128,  $\theta$  303,  $\chi$  44, all badly attested lines.<sup>2</sup> Then follows (p. 271) the summation: "Accordingly we can state that in a number of places the external evidence is refuted and annihilated by the internal evidence." I must disagree.

"In the domain of metrics and euphony we meet with traces of Aristarchian activity. In  $\iota$  222 the vulgate gives:

$\chi\omega\rho\varsigma \delta' a\bar{\imath}\theta' \epsilon\rho\sigma\alpha\iota \cdot v\bar{a}\nu\delta' \dot{\alpha}\rho\bar{\omega} \ddot{\alpha}\gamma\gamma\alpha \pi\acute{a}v\tau\alpha$

while Aristarch reads  $vai\bar{o}\nu$ . In Homer  $vai\bar{w}$  'live' and  $v\bar{a}\omega$  'flow' occur. The latter always has a short vowel ( $a$ ), whereas in  $\iota$  222 the metre requires a long  $a$ . As we know that in Homer short vowels can be used as long ones, when the metre demands this,  $v\bar{a}\omega$  with long alpha cannot surprise us. This fact was not yet sufficiently known by Aristarch, who therefore unnecessarily emended the text. This is, however, not necessary."

This recalls the days of my boyhood, when *metri gratia* was as potent as "Open Sesame." Since then, thanks first and foremost to Wilhelm Schulze, it has been learned that metrical lengthening is found only in certain definite circumstances.

The passage is difficult. Chantraine<sup>3</sup> posits  $vai\bar{w} < *vaf-y\omega$  along-

<sup>2</sup> In passing,  $\psi$  127-128 are not in the papyrus said to contain them. The same applies to the mention (p. 279) of  $\Lambda$  543.

<sup>3</sup> *Grammaire homérique* (Paris, 1942), p. 164.

side of *vāw* < \**vaf*-*w*. This is easily possible, but does not account for the rarity of *vaiw* "flow." I am more inclined to associate the trouble with a phenomenon discussed by Cauer.<sup>4</sup> At an early time half-learned copyists tried to smooth out what seemed to them metrical defects. If they made things worse, there followed efforts to correct their corrections. I would read with van Herwerden ἔρσαι· ἔναον. This was changed to ἔρσαι· *vao*n to do away with the (unobjectionable) hiatus without concern about the quantity of the alpha. Later (probably before Aristarchus) the latter difficulty was rectified by writing *vai*o*n*. Both *vāo*n and *vai*o*n* were known in later antiquity, and both are still found in the vulgate manuscripts. Aristarchus read *vai*o*n*, and is followed by Ludwich, Allen, and von der Mühl.

Metric factors are also involved in a group of passages, treated, pp. 45-46, to justify the combination of -δε and εἰς with an accusative.<sup>5</sup> Aristophanes read:

*ρ* 52      αὐτὰρ ἐγὼν ἀγορήνδε ἐλεύσομαι ὅφρα καλέσσω

perfectly good metre, in spite of the hiatus. To eliminate this ἀγορήνδε ἐσελεύσομαι was introduced, and probably accepted by Aristarchus. Later another correction ἀγορὴν ἐσελεύσομαι appears and becomes predominant in the vulgate. Von der Mühl is right in following Aristophanes. Then similar action must be taken at a 88 Ἰθακῆνδε ἐλεύσομαι though the reading of Aristophanes is not attested. In a 285 κείθεν δὲ Σπάρτην δὲ dittography has led some MSS to δ' ἐς Σπάρτηνδε; but Allen rightly thinks it not worth mentioning. In φ 58 the MSS give a choice between βῆ ρ' ἵμεναι μέγαρόνδε and ἵμεν εἰς which should not be difficult. This leaves κ 351:

ἔκ θ' ἱερῶν ποταμῶν οἵ τε ἄλαδε προρέοντι (Zen.)  
οἵ τ' εἰς ἄλαδε προρέοντι (Ar. Ω)

The choice is between bad metre and bad grammar. Ludwich, Allen, and the author follow Aristarchus; von der Mühl does the same in his text, but with a note "recte ut videtur" attached to Zenodotus' reading. I agree that this is the oldest form of the text known to us; but regard the metre as indicating either that Zenodotus' text was already corrupt, or that the line is interpolated. Aristarchus read:

*δ* 37      ὡς φάθ'. ὁ δὲ μεγάροιο διέσσυτο, κέκλετο δ' ἄλλους

and is rightly followed by Ludwich, Allen, and von der Mühl. Some one, not understanding the lengthening before *μεγάροιο* wrote ὁ δ' ἐκ *μεγάροιο* to improve the metre, and the MSS so read. Didymus gives us a *terminus ante quem*.

More interesting is:

*ι* 283      νέα μέν μοι κατέαξε Ποσειδάων ἐνοσίχθων

so read by Aristarchus, followed by Ludwich, Allen, and von der

<sup>4</sup> *Grundfragen der Homerkritik* (3te Auflage, Berlin, 1921), pp. 74-77, cf. p. 68.

<sup>5</sup> Van Leeuwen, *Enchiridium Dictionis Epicae* (Leyden, 1918), p. 157, seems to recognize only κ 351.

Mühl. Most, not all, MSS read *vῆa*. The author thinks it possible "that Aristarch has made a conjecture which hits the mark," in spite of the fact that "Homer always gives the form *vῆa*." All that the tradition gives us is that the text when written in Old Attic script was NEAMEN; interpreting that either as *vῆa μέν* or *vέa μέν* is no conjecture. Neither is metrically possible, and attempts have been made to get a *στίχος λαγαρός* either *vῆa μέν μοι [κατ]έαξε* (Schulze), or *vῆa μέν [μοι] κατέαξε* (von der Mühl). A third transliteration is possible, and I prefer to follow van Gent, and read *vῆ̄ ἀμμῆν [μοι] κατέαξε*.

The example chosen (p. 38) to show how "the vulgate sometimes has preserved curious items of the archaic mentality" is:

ξ 202 γνήσιοι ἐξ ἀλόχον· ἐμὲ δ' ὠνητὴ τέκε μήτηρ.

So read by Ludwich, Allen, and von der Mühl with a goodly minority of the MSS; while the author believes that *ἀλόχων* read by the majority is original. His argument is that this Cretan noble was being spoken of as a polygamist. I look upon that as improbable: for (1) the Achaeans are not described as polygamous; and (2) wives of monogamists sometimes die, and sometimes their relicts find consolation. The plural *ἀλόχων* would never have troubled anyone; but *ἀλόχον* creates a hiatus seemingly awful, though actually unobjectionable. Ludwich's commentary shows *ἀλόχον* changed to *ἀλόχων*, but not the reverse.<sup>6</sup> Probably the corruption happened quite late.

The author does not always handle linguistic problems in the way I should treat them. For instance of *ἀπονίπτεσθαι* read by all MSS in σ 179 he says (p. 71): "In σ 179 by the side of *νίζειν*, which is found elsewhere in Homer, the infinitive *νίπτειν* occurs. Attic only knows *νίζειν*, whereas there is no reference of *νίπτειν* before the Koine. It is, however, very well possible that such forms occurred already in the time of Homer and have not been developed in Attic." A very faint possibility cannot be denied;<sup>7</sup> but Apollonius Sophista read *ἀπονίψασθαι* (recte fortasse, von der Mühl), and I should follow him or follow Cobet in removing the line. Wackernagel puts it in a nutshell:<sup>8</sup> "Wer an dem -*νίπτεσθαι* festhält, muss in dem Vers eine ganz junge Zuthat sehen." Chantraine (*Gram. hom.*, p. 334) calls attention to the lateness of σ, but that probably does not go far enough.

On p. 70 the author argues that *κέχανδα* [found in all MSS at δ 96, Ψ 268, Ω 192, attested by Apollonius Sophista, and cited by Didymus from Aristarchus] is "no textual corruption," but a genuine Homeric form analogic to *χανδάνω*, *ἔχαδον*. That is its origin, but the date of its creation is open to question. Fick and Wackernagel pointed out that for Homer *κέχονδα* is to be expected; and years afterwards a papyrus (P 13) of the 1st century B. C. was found containing it. By that time, then, the form had not yet been driven out of the tradition entirely; and since no one could have ever

<sup>6</sup> Ludwich cites 5 MSS for *ἀλόχων*; 4 (2 with *ων* written above) for *ἀλόχον*.

<sup>7</sup> For the broader setting of the problem, cf. Schwyzer, *Griechische Grammatik* (München, 1939), p. 704.

<sup>8</sup> *Sprachliche Untersuchungen zu Homer* (Göttingen, 1916), p. 74.

dreamed of inserting it, it must have been there at the start. In an Addendum the author brushes off this evidence: "Linguists have wrongly put trust in the reading of the papyrus." Pasquali<sup>9</sup> chooses this as his second example, and says: "Bastano tre novità digital fatta a mostrare che i papiri possono offrire lezioni superiori alla tradizione medievale e delle quali nella tradizione medievale non è rimasta traccia." He then adds half a dozen other examples and refers to a goodly number collected by others.

There is a needless effort (pp. 70-71) to show that the sound represented by  $\varsigma$  was lost at an early date in Ionia. Then follows an inference that  $\lambda\ 442$  containing  $\mu\eta\delta\ \text{oi}$  is Homeric. Wackernagel<sup>10</sup> has shown that even in the 5th century no Ionian would talk that way.

The author shares with Wilamowitz<sup>11</sup> a belief that Aristarchus' error of not recognizing an obsolete  $\iota\sigma\kappa\epsilon$  "he spoke" in  $\tau\ 203$  led him into the further error of athetizing  $\chi\ 31-33$ . Wilamowitz is content to be merely pontifical: "Dass er in Wahrheit ein sehr altes Wort strich, konnte er nicht wohl wissen, aber wir wissen es." The author undertakes (p. 116, n.) to prove the existence of  $\iota\sigma\kappa\omega$  "to speak" by the "related ( $\dot{\epsilon}\nu$ )  $\iota\sigma\pi\omega$  which occurs in Homer." There is no  $*\iota\sigma\pi\omega$ . The author has got hold of  $\dot{\epsilon}\nu\text{-}\sigma\pi\epsilon\iota\upsilon$ , the aorist of  $\dot{\epsilon}\nu\text{-}\epsilon\pi\omega$ , the subjunctive of which  $\dot{\epsilon}\nu\text{-}\sigma\pi\omega$  occurs. He finds it curious that Bechtel<sup>12</sup> and Chantraine<sup>13</sup> should "have been led astray by Aristarchus." I go along with them.

Finally there is (p. 63) confusion of speech and writing. The noises Homer made sounded just as sweet as they did sound, no matter how they were recorded. Through the 6th and 5th centuries the Ionians, as their inscriptions show, wrote EO for whatever noise resulted from the contraction of the noises symbolized by E and O; only about 400 B. C. did they start using EY to represent this sound. When a choice between the contracted and uncontracted sounds was open to him, Homer, one may believe, was guided by his likes and dislikes for certain sequences of noises (euphony). That has nothing to do with the way they were represented in writing.

Such views of language make it easier to understand how the author can attribute the *Iliad* and/or the *Odyssey* to one poet.

G. M. BOLLING.

OHIO STATE UNIVERSITY.

F. A. LEPPER. Trajan's Parthian War. London, Geoffrey Cumberlege, Oxford Univ. Press, 1948. Pp. xv + 224; map. \$5.00.

The triumphant expedition of Trajan, which carried the limits of the Roman empire to the greatest extent they ever reached, has naturally engaged the interest of modern historians, attracted by the

<sup>9</sup> *Storia della tradizione e critica del testo* (Firenze, 1934), p. 242.

<sup>10</sup> *Op. cit.*, pp. 107-109.

<sup>11</sup> *Die Heimkehr des Odysseus* (Berlin, 1927), p. 62.

<sup>12</sup> *Lexilogus zu Homer* (Halle a. d. S., 1914), p. 182.

<sup>13</sup> *Op. cit.* (above, n. 3), p. 317.

importance of the operation and the problems posed by a fragmentary and contradictory tradition. In 1937 Julien Guey published in Bucharest an *Essai sur la guerre parthique de Trajan* which scrutinized the sources so assiduously that one's first reaction is surprise to find another monograph on the same subject following it so shortly. But the author's preface makes it clear that his purpose is not to do Guey's work all over again. Rather this book is an extensive review and critique of Guey and of Longden's account of the Parthian war, both in an article in *J.R.S.* for 1931 and five years later in Volume XI of the *Cambridge Ancient History*. As a statement of the problem and an investigation of specific questions it deserves the heartiest commendation.

After an introduction which gives a very good résumé of the ancient and modern authorities, Part I deals with the chronological problem. This is an examination of the literary sources, combined with an assembly of the evidence of coins and inscriptions. In the first category chief attention is given to John Malalas who dates the third earthquake at Antioch on Sunday, December 13, 115. We know that Trajan spent the winter at Antioch between the capture of Batnae and the campaign against Adiabene. If Malalas is right that the great earthquake during Trajan's visit was in the winter of 115/16, the war occupied three campaigns and Ctesiphon was not captured until 116, which is the theory of Guey which Longden rejects. Malalas calculated by the 164th year of Antioch, the second year of Trajan's stay there, and the consulship of M. Pedo Vergilianus. The elements of this compound date are consistent except for the day of the week, which is wrong. Unconvincing attempts have been made to explain this error, in the belief that the whole compound date must stand or fall together. Longden rejects the explanations and the date. Lepper argues that the various elements come from different sources; if these elements are consistent the probability of correctness as a whole is increased. On the other hand, a single inconsistent element does not necessarily vitiate the whole, since it may be derived from a bad source or may be merely one of Malalas' characteristic blunders. Lepper therefore concludes that the day of the week is wrong but that the true date is, none the less, December 13, 115. This conclusion is the result of thirty-two pages of the most detailed scrutiny which is probably as sound an analysis of Malalas as can be applied to that annoying author. It is questionable how much his plausibility is increased by the discussion of his uses of the year of Antioch (three right, three wrong, ten suitable but impossible to verify) and of Macedonian months (twenty-seven suitable but impossible to verify). Lepper seems to hope that we will count the suitable instances to Malalas' credit. I doubt if this is statistically admissible. Nonetheless, the chapter inspires much more confidence than von Gutschmid's postulate of three sources, or von Stauffenberg's emending away of difficulties, or Longden's rejection of Malalas as hopeless. Hopeless he certainly is at times, but there are some fish in that muddy source and Lepper shows how a careful man may go about catching them.

The evidence of the coins and inscriptions is presented in two tables, one concerning the date of the title "Optimus," the other that of "Parthicus." The tables would be easier to use if they did

not include material which is now out of date or which is not really cogent, and if the periods in the keys were not so hard to distinguish on the tables. For example, Strack's "Optimo Principi Coins" cited ("Nos. 78 f., 357 f.") and dated 103-112 on Table I must be those pieces where the reverse has SPQR OPTIMO PRINCIPI but "Optimus" is not part of the title (Nos. 78-113, 116-163, 165-179, 184-197, gold and silver; 357-433, bronze). But since the subject of Table I is the title "Optimus" these are not pertinent. The imperial coins which really bear on the question are Strack's No. 218-260, gold and silver; 448-479, bronze, where the obverse inscription has TRAIANO OPTIMO. The evidence of these coins on the question of the date is not shown in the table at all. Since they are all associated with COS VI, they cannot have been earlier than 112. Since Nos. 171-197, 202-217a, 261f., 418-440 have COS VI but not "Optimus," it is reasonable to put these issues between January 1, 112 and the conferring of the new title, which would provide a suitable body of coinage if the date is the autumn of 114 (p. 38). The imperial coinage, then, confirms the inscriptions, but I do not see how the reader could make that out from Table I.

E. A. Sydenham's monograph, *The Coinage of Caesarea in Cappadocia* (London, 1933), should be cited in this connection. The types dated by the sixth consulship only are 39 in number: 8 tridrachms, 22 didrachms, 9 drachms. If the minor varieties are consolidated, there are 3 types of tridrachm, 14 of didrachm, 7 of drachm. Of these none of the tridrachms, 6 didrachms, and 5 drachms have "Optimus" in the title. But a drachm of TR POT XVIII (p. 65, No. 189) does not have it. Since the date runs from December 10, 114, and the die may have been sunk in anticipation of the event, this makes no serious problem for the conferring of "Optimus" in the fall of 114. No coin of Caesarea bears the title "Parthicus." Inspection of all the Caesarean issues suggests that the issue of tridrachms, begun in 98, had ceased by 114, with the didrachms somewhat decreased and the drachms increased between the fall of 114 and February 116. In 116 and 117 the mint was apparently idle. All this sheds some light on the financing of Trajan's operations in the north. It is not without interest to remark that the title "Optimus" occurs very rarely from any of the northern mints except Caesarea (only Amastis and Heraclea so far as I know) and "Parthicus" never, whereas both titles are found in the south. It certainly seems as though the center of activity had shifted away from the north before "Optimus" was conferred. Of course, all the northern coins except those of Caesarea are of bronze, and bronze is primarily civil and not military money. An increase in issues of bronze, therefore, does not give direct evidence that there were soldiers in the region to be paid, nor does the cessation of such issues necessarily mean that the soldiers have moved away. Such phenomena might be, and generally are, quite independent of any military movements. But the presence of an army does have an inevitable effect on the tempo of business, and when it moves away the bronze already in circulation may be sufficient for the reduced needs of the civilians. Granted the known presence of Trajan's army in northern Asia Minor, the frequency of issues without "Optimus" and the scarcity of those with that title may fairly be taken as evidence that the army had left before the title was conferred.

In the section on chronology all the problems are exhaustively discussed whether they are capable of solution or not. The resultant conclusions are given on pp. 95f. The merit of the pages preceding is that they supersede all previous treatments and furnish a basis on which any future evidence may be judged—and new evidence, epigraphical and numismatic, does still appear from time to time.

The section on strategy and topography is not so complete, but more original and more important. The first large question is what to do about the year 115, which is full of imperial salutations but very bare of recorded events. For the silence of the literary sources there is no cure, but Lepper has resorted to a detailed study of the Eastern limes, and has advanced the attractive theory that 115 was spent by Trajan in establishing the frontier on the general line later restored by Diocletian. He believes that the importance of that work was so generally felt as amounting to a settlement of the Eastern question that the triumphal quality of the year is explained, though its incidents are lost. The loss is no cause for surprise considering the state of the tradition; there is reason to believe that there was once an extended account by Arrian. As to the campaign of 116, it is suggested that the attack on Adiabene and the capture of Ctesiphon may have been "two almost distinct and independent consequences of the arrangements of 115." This is certainly not made impossible by the sources, and it allows us to retain the evidence for Trajan's using of the Euphrates route. If the movements were simultaneous the advantage of speed may have compensated for the difficulties of the desert. (Lepper does not say that they were simultaneous, but if Adiabene was finished before the march to Ctesiphon began I am still doubtful whether a shift to the Euphrates can be accounted for by any facts we possess.)

To the argument that Trajan annexed all of Armenia in 114/15 a scrap of evidence may be added from Procopius (VIII, 2, 16) who reports a tradition that detachments under Trajan were stationed east of Trapezus as far as the Lazi and Sanigae, that is, all along the coast flanking Armenia Major, affording exactly the kind of protection that Lepper considers practicable.

As to Mesopotamia, his basic hypothesis is that it was bounded on the south by a line running from the Chabores to Singara, and that at no time did it include Parapotamia, west of the Euphrates, Adiabene, east of the Tigris, or Babylon to the south. The organization of the province of Mesopotamia was the achievement of 115 of which the advances of the next year were no necessary part, whatever their cause. Dura in Parapotamia, being outside this frontier, may then have been ceded to Parthamaspates at his coronation in 116/17, and the Roman withdrawal from it need not by any means imply an intent to abandon Mesopotamia, as Rostovtzeff has argued on the basis of an inscription which gives specific record of that withdrawal. This has the advantage of explaining the single piece of epigraphical evidence without necessitating an entire reversal of the conception of Trajan's policy in his last year. But Lepper's suggestion that Dura may have participated in the revolt of 116 and been pillaged in consequence is quite unnecessary. The sole instance of pillage is the removal of the doors from a small temple which had to be replaced after the Romans left. All the other

evidence points to an unbroken continuance of normal civic life. There is nothing to show that the Romans did not take the doors for use in some other structure in the town—a proceeding likely enough in country where wood is so scarce, whereas the supposition that they left town carrying the doors with them is obviously a shaky one. Nor is it likely that the Romans pillaged Dura and left it because "this city, which was not so much a stronghold as a trading-post, might not have seemed worth retaining." We know very little of its value as a trading-post in the second century, but that it was an effective stronghold its history in the third century clearly shows. On the other hand, its peaceful cession to Parthamaspates would fit the rest of the evidence very well. Lepper's hypothesis has a great deal to commend it.

It is important to the theory that Trajan's chief motive was desire for a permanent frontier to show that the war was pre-meditated. Lepper's conscience, however, forces him to conclude that, in spite of the conviction of some earlier scholars, there is no real evidence of premeditation as yet available. Here he could get important support from the silver coins of the East. Both in Cappadocia and in Syria the activity of the mints is worth studying. Caesarea had long been used for the issue of didrachms, drachms, and hemidrachms. Under Vespasian there had been 5 types of the first, 6 of the second, 3 of the third. The reign of Titus seems to have produced only a single type of hemidrachm, but under Domitian there were 9 types of didrachm, though only one drachm and no hemidrachm. So far there is no cause for surprise, but the year's reign of Nerva produced 12 types of didrachms, and by 114 Trajan had struck 11 types of didrachms, 8 of drachms, one hemidrachm, and 6 types of tridrachms designed to take the place of the traditional Asiatic eistophorus. Clearly Trajan had inherited from Nerva a policy of increased production in Cappadocia. One does not think of Nerva as an expansionist, but we really know nothing at all about his policy in Asia. Of course, the issue of silver money at a mint other than Rome is not an infallible sign of preparation for war, but that is an explanation that must not be overlooked. For Trajan's performance at Caesarea entirely suits the theory that his silver was military money. Not only does the supply dry up when the northern campaign is over, as already remarked, but there are two things which make it apparent that the issues had imperial and not merely local significance. The first is the appearance of Arabia as a type with COS V at Caesarea simultaneously with its appearance at Rome, and certainly it must have had a strange look to the Cappadocians. The second is that the Cappadocian silver does actually leave its proper territory, and turns up in surprising quantity in Syria (*Dura Final Report*, VI, p. 204). Since Syria had an abundance of silver of its own, the obvious explanation of the intrusion is the military one, and if soldiers did transport the silver of Caesarea there is no reason to doubt that they were intended to do so. It is a principle not always recognized that money for a campaign is used for the preliminaries as well as for the actual fighting, and that the pattern of a perfectly planned campaign would be a significant increase of production before the outbreak of hostilities, with a decrease thereafter and a return to normal when the war is over.

This is just what we find at Caesarea except that no one, so far as I know, has ever suggested that preparations for war were begun under Nerva.

The situation in Syria is similar and without any unexpected elements. There Nerva struck only one type of tetradrachm whereas before 114 Trajan struck 23! Much the commonest are the three issues of 110/11, and there is a marked falling off in 115-117. He did not use the mint of Cyprus for silver as the Flavians had, nor did he strike bronze there until his sixth consulship, so that no general increase of business of the Near East is to be assumed. On the other hand, he seems to have used Tyre as well as Antioch (at least he struck Tyrian types) and Newell has shown that Wroth's No. 154 of 107/8 was struck in Egypt—an unexplained intrusion of good silver into the territory of base. All this is clearly preparation for something, and if not for the war which actually followed, then for what?

Lepper gives a good summary of his conclusions at the end. While much is of necessity hypothetical, it is the most persuasive reconstruction of motives and strategy that has yet appeared, and solidly based on the painstaking analysis of the preceding chapters.

ALFRED R. BELLINGER.

YALE UNIVERSITY.

---

FRIEDRICH SOLMSEN. *Hesiod and Aeschylus*. Ithaca, N. Y., Cornell University Press, 1949. Pp. ix + 230. \$3.00. (*Cornell Studies in Classical Philology*, Vol. XXX.)

The debt of Aeschylus to Hesiod, as well as to Homer and Solon and other poets, has always been realized, through never thoroughly explored; yet only a just appreciation of the debt can enable us to measure the originality of Aeschylus. Professor Solmsen's careful study is therefore most welcome; it goes far toward providing the reader with the necessary orientation. One has the feeling that the development of Greek poetry moves with the gradual but inevitable power of an incoming tide, the waves now creeping forward, now retreating, but at last gathering in full flood the sum of many minor gains.

Something of this impression may be felt also in the movement of thought of Solmsen's book. It does not set forth at the start a thesis to be defended, but poses problems, suggests answers or hypotheses, considers alternatives, moves on to new problems, qualifies statements or concedes rival explanations, achieves new vantage points, and pauses from time to time to survey the territory gained. As to certain prior assumptions he does not so much argue a case as indicate briefly his own position, letting the sequel show that it provides a consistent explanation of the facts: thus he sits loosely to the Homeric question, but appears to be a tacit chorizontist; he accepts both the *Theogony* and the *Works and Days* as poems by Hesiod (and as composed in that order); he cautiously distinguishes the original text of Hesiod from later accretions; he deals with the

text tradition of the various poets and with proposed emendations judiciously and with tact. In considering these matters and the larger issues of substance and thought he naturally draws on a very considerable modern literature, and seems to have overlooked few pertinent recent works. (He might, however, have used J. A. K. Thomson, "The Religious Background of the *Prometheus Vinctus*," *H. S. C. P.*, XXXI [1920], pp. 1-37.) A valuable part of his work consists in sober and acute comment, offered with a pleasing mingling of modesty and independence, on the interpretations of previous scholars, sometimes in the text, more often in the 857 footnotes.

To criticize here in detail so intricate a mass of material would be impossible; suffice it to say that the reviewer finds Solmsen's major observations both valid and illuminating, and would venture to disagree with him on only a few minor points. It is more profitable to attempt to call attention to some of the chief points made in the book.

Hesiod knew parts of Homer, but perhaps not all the "late" parts; he is more systematic, less static, in his theology than Homer, whose chief contribution to the ideas that interested Hesiod are the eminence of Zeus, the division of provinces among three gods (in the "late" *Apate*, *Il.*, XV, 185-195), and the abode of Cronus and other Titans in Tartarus. It is Hesiod who works out the succession of three generations of divinities (Uranus and Gaea, Cronus, Zeus), and who distinguishes powers of good and evil among previous ideas and abstractions; some are now associated with Zeus by his "begetting" or "winning" or "honoring" them. Groups of beings are differentiated and acquire personality (Titans, Muses, etc.). Earlier stories are manipulated by Hesiod in order to enhance the power of Zeus: Prometheus, the fates of the children of the Titans, and the victory of Zeus over the Titans. The marriages of Zeus and his children are essential to the new world order; for the *Theogony* begins as cosmology but passes into the realm of ethics, as old and new figures are joined. Mnemosyne is "the first divine incarnation of a spiritual power as such" (p. 70), and like other Hesiodic beings derives her status not from genealogy but from intrinsic worth. Thus Hesiod is both original and conservative: "his history of the gods culminates in the ascendancy of Zeus, but it provides for the incorporation and integration of some older forces and deities—those whose character allowed it—into the new dispensation" (p. 75), a point important to remember in considering the conclusion of the *Eumenides*.

The *Works and Days*, though less abstruse than the *Theogony*, includes genealogical speculation, as the *Theogony* includes the world of the present; a speech, not a narrative, it consists largely in Homeric *paraenesis*, utilizing as a motive the good kind of Eris, not merely the evil Eris of the earlier poem. In dealing with the problem of evil the poet is still a misogynist, and therefore a fatalist, in viewing the evils brought by Woman; but some freedom of the will is to be discerned in the picture of the Five Ages. If Hesiod is not a social reformer, and accepts the peasant's hard lot, he craves justice, and believes that the rewards of *hybris* and of *dike* are to be reckoned with, though difficult to discern; Dike finds a place in the family of Zeus, and not the evils. Thus the poet is not a complete

pessimist; and the concern of Zeus with human justice, an idea rarely found in Homer, "is the poet Hesiod's great contribution . . . to Greek humanism" (p. 96).

An important link between the thought of Hesiod and that of Aeschylus is Solon, who added to his personal observation of the questionable value of wealth gained unjustly a reading of Hesiod; for "the relationship which Hesiod had established between *hybris*, wealth, the punishment of Zeus, and Ate had become canonical" (p. 109). Solon, to be sure, placed a new emphasis, in his first elegy, on Hope, and on the uncertainty of the end given by God (Zeus or Moira); thus his belief in just retribution is qualified, at least for some sectors of experience. Elsewhere, especially in his third Elegy, he draws similar conclusions for the city; now it is not the spontaneous act of Zeus but the "immanent causality" of Dike that avenges, and Solon's pedigree of evils substitutes the present tense for the aorist appropriate to Hesiod's genealogical account. Here we meet empirical laws, a regular sequence of political development: in a word, the germ of a science of polities. Like Hesiod, Solon does not introduce *hybris* and *koros* into the Greek pantheon, for they are the result of human activity; but Dike and Eunomie, who in Hesiod are sisters, are taken over, together with Eirene, only partly secularized. They provide a religious sanction for those political activities of Solon which sought by moderation to give effect to justice and order.

Solmsen devotes his last hundred pages to Aeschylus, naturally dealing chiefly with the *Prometheia* and the *Eumenides*. In setting forth the conflict of Zeus and Prometheus, Aeschylus uses Hesiod's conception of the three generations of divinities, and some of the Hesiodic accretions: the delivery of Prometheus by Heracles, and the last revolt of Typhoeus against Zeus; not the interpolated Metis episode (127; 161; *Theog.* 886-900), but rather (with Pindar) the danger to the security of Zeus in his contemplated marriage with Thetis. Hesiod, despite his concern with Dike, does not apply the conception of justice to the Prometheus myth; Aeschylus is troubled by the antinomy of the might of Zeus and the injustice of Zeus to Prometheus and to man. The Zeus of *P. V.* is not so much a political figure as an angry individual, inaccessible to Peitho and to pity. The Prometheus is cunning; though the Hesiodic tale of the tricking of Zeus at the sacrifice is ignored, the story of the second outwitting in the theft of fire and the founding of human crafts and civilization are stressed in order to give Prometheus tragic dignity, and in order to enhance the contrast between man's primitive insignificance and his present estate. Thus from Hesiod's few lines on the punishment of Prometheus grows the vision of Aeschylus. Yet the Zeus of this play is not at all the Zeus of the *Suppliants*, the *Persians*, and the *Oresteia*, stable, just, the vindicator of justice; nor, it should be added, is there here any ethical conception of man. In short, this is not yet a moral universe, but rather the arena of conflict from which emerges secular and technical achievement. Prometheus, too, though "philanthropic," errs through self-righteousness, so that like certain other Aeschylean trilogies the *Prometheia* begins with the conflict of parties both of whom are guilty of excess, and can achieve a final harmony only through concessions on both sides. We are

lamentably ignorant of most of the details in the sequel; but it seems clear that, whereas Hesiod thought of the ordeal of Prometheus as eternal, and an interpolator attributes the delivery of Prometheus through Heracles to the desire of Zeus to honor his son (*Theog.* 530 ff.), Aeschylus shows at the outset of the *P. Solutus* a milder Zeus who has already released the Titans and who is capable of making further concessions. So it appears that Aeschylus accepts from Hesiod the idea of a powerful but immoral Zeus only for the initial stage of his reign, and sees divine justice emerging in divine history, as in the *Eumenides* and indeed in Athenian history justice was hardly won; Solmsen thinks it not too hazardous to suppose that the trilogy showed a corresponding enhancement of human dignity and morality (p. 154). Aeschylus has carried further than Hesiod the Hesiodic pattern of divine growth and reconciliation.

The new spirit is manifested in the handling of the crisis of the Olympian dynasty. From the scheme of the three divine generations and from the idea of vengeance and the Erinyes and of curses to be found there, Aeschylus interpolated the curse of Cronus on Zeus when he fell (cf. similar curses in the stories of the Labdacidae and the Atridae). But though tyrannical Zeus and his hybristic deeds are not immune to vengeance, all tradition testifies to his escape and the stability of his reign. The danger of Zeus Aeschylus attributes to the self-willed character of his early reign, his escape to its changed policy, which nullifies the curse of Cronus. So here, as in the *Eumenides*, Dike supersedes the Erinyes. Since the new idea does not play a part in the Theban trilogy, nor in any other (unless in a different sense) except the *Oresteia*, the question arises whether the poet worked from the exploration of the form of the trilogy to the perception that Homer and Hesiod illustrated them, or whether the substance and ideas of these poets prompted the construction of the trilogy. Solmsen suggests that both trends interacted; he refers to other "third plays" ending in reconciliation, peace, persuasion, and divine intervention. The cardinal instance, of course, is the *Eumenides*, which ends "under the auspices of divine Justice, a Justice whose significance increased for Aeschylus when he became convinced that the right order which the gods established on Earth was the same one that had saved their own government from a fatal crisis and impending catastrophe" (p. 177). To this play the final chapter is devoted.

Here again we contemplate "old gods" and "new gods." But Aeschylus, and not Hesiod, makes the Erinyes the "daughters of Night." In Hesiod, Night is mother of various Evils, while the Erinyes spring from the blood of Uranus at the time of the attack of Cronus; in other words, they arise at the first act that calls for their activity and a father's curse on his son. The Erinyes of Aeschylus stand for a certain kind of "order" or "law," but only by the merciless persecution of evil-doers; his sense of a conflict between them and the new moral order of Zeus is due less to his awareness of any acute contrast between a matrilineal and a patrilineal organization of society and economic implications (*pace* G. Thomson) than to his brooding on Hesiod. The conflict is not definite in the *Agamemnon* and the *Liberation Bearers*, though their choruses prepare us for the changed view of the moral standing of Orestes,

contrasted with that of Clytemnestra, that comes out in the *Eumenides*. Solmsen takes a more sympathetic view than many scholars of the arguments and "hard reasoning" of the last play. The tie vote of the jury is indeed a maximum recognition of the claims of the Erinyes, in a democratic process; and at last they are persuaded and incorporated in the new order, not crushed. Inclusion of older deities in the dispensation of Zeus was a Hesiodic pattern of thought; but Aeschylus goes further in showing that the Erinyes have actually a moral function and a "capacity for spiritual assimilation" (p. 199). So as Semnai, or Eumenides, they become beneficent as well as dreadful, and extend their province to include all human affairs. (In the *Prometheia*, one might say that Zeus stooped to conquer; here the Erinyes do likewise.)

The founding of the Areopagus is represented by the poet as raising Athens to a new political and moral status; Athena herself says as much in her solemn and beautiful charge to the citizens, in which she invokes the Solonian "mean," supported by awe. "The whole passage is the last great apotheosis of Dike in Greek poetry" (p. 211); and the Erinyes, now spirits both stern and kindly, sing their song of benediction. The family feud has been resolved by being transferred to the *polis*, which is immune from the curses and the fates of individuals; or, in other terms, the antagonism between Hesiod's "Children of Night" and the younger gods has been restated by being embodied in Athenian institutions and morality. Aeschylus does not shift or confuse the issue by using Athens to solve the problem; rather he shows that divine justice may operate through human agencies. Hesiod has incorporated basic social and political values in the family of Zeus; Solon has investigated *hybris* and *ate* in personal life and *dike* in the community; Aeschylus exhibits these forces at work in society, showing Dike not only as punishing the guilty individual but as preserving the city.

Such are some of the points made by Solmsen in the course of his stimulating book. There are many more, doubtless some of them quite as important as those here selected for mention. Possibly the author's absorption in Hesiod has eclipsed the still greater figure of Homer; yet it remains true that Aeschylus could not have digested the *temache* of the great banquet as he did without the services of Hesiod. Here, at any rate, is another book worthy to stand beside the author's *Plato's Theology* (*Cornell Studies*, Vol. XXVII [1942]).

WILLIAM C. GREENE.

HARVARD UNIVERSITY.

GEORGE KARO. *Greek Personality in Archaic Sculpture*. Cambridge, Mass., published for Oberlin College by Harvard University Press, 1948. Pp. xvii + 343; 32 pls. \$4.00. (*Martin Classical Lectures*, XI.)

This book is a masterpiece. It is the mature fruit of a long life of a scholar who for many years has lived among and for the study of the works of early Greek art, which he discusses here. He sees

sculpture as the reflection of personalities whose character and ideas are expressed in them. Although many excellent books and papers have appeared in the last fifty years in the previously neglected field of archaic art this is the first comprehensive study of the whole development from the Mycenaean age to the geometric, the Orientalizing, the Daedalic, to the sixth century archaic period. It is also the first time that in each period the various geographical regions are clearly and convincingly distinguished from one another, not only Doric, Ionie, Attic, but also different cities and landscapes, like Argos from Corinth, Sicyon, and Laconia; the Cyclades from Samos and Miletus from Ephesos and Chios. All older attempts in this direction are here surpassed. Further investigations and excavations may bring modifications, but certainly no decisive changes. The most outstanding books in this field, like the admirable *Kouroi* by Gisela Richter, do not take into account the regional differences, or deal, like the *Kouroi* and the *Archaic Attic Gravestones* by the same author in the same series of *Martin Classical Lectures* (Vol. X), with just one group of monuments from one place. In Karo's book we have a synthesis which only a life-long study and devotion could produce.

Each period and each group is characterized with concise and striking words. The earliest phase of Greek sculpture is seen in the mainland Mycenaean art with its already truly Hellenic feeling for tectonic values, structure, and proportions in contrast to the pictorial expression of fugitive movements in the Cretan-Minoan art. It follows the geometric style with its tense vitality bound by stern discipline (Ch. II). The Orientalizing period in the late eighth century produced a new Greek personality, when imported oriental works and travel to foreign countries released new creative forces in the Greek artist. Eastern Ionia, Dorian mainland, Crete, and Attica react in characteristic and diverse manner (Ch. III). This period is less homogeneous than the preceding or following period, according to the stronger or lesser resistance to the foreign influence. An unimpaired purely Greek style is found only in the so-called Daedalic style of the seventh century. Its chief characteristic is an austere purity, and the artists were probably mostly Doriāns (Ch. IV). The earliest great statue, dedicated to Artemis by Nikandre of Naxos at Delos, the bronze figures of Apollo, Artemis, and Leto at Dreros, in the sphyrelaton technique, that is thin bronze plates riveted over a wooden core, and the sculptures from Prinias, both in Crete, are the most important examples.

The archaic sculpture in a narrower sense, that of the sixth century, is treated in the three main chapters on Dorian, Ionian, and Attic Sculpture (Chs. V-VII, pp. 103-281). It is the period of the Kouroi. Karo shows how the Dorian Argive Twins, Kleobis and Biton, all "brawn and muscle" over a powerful skeletal structure, are charged with intense force (p. 109). The Ionian Naxian Kouros is less heavy (p. 184). The Samian Kouroi have a smoothly polished surface showing that the Ionian artists were interested more in the outward aspect of the human forms (pp. 199 f.). The Attic Kouroi are pictures of the wealthy Eupatrids, the well-born landowners and aristocrats (pp. 250 ff.), as Gisela Richter had already seen. Similar differences appear in all other subjects. The pedimental reliefs

from the temple of Artemis in Coreyra reflect the artistic development of its mother city Corinth (pp. 112 ff.), while the poros pediments from the Acropolis are typically Attic with their feeling for monumentality and vitality in the figures of Heracles, monsters, and beasts and their advance in plastic values (pp. 244 ff.). New discoveries like the finds from Perachora are given their place in the history of art (pp. 120 ff.). Others, long known, are assigned with good reasons to a definite place, like the Hera from her temple in Olympia to Corinth (pp. 127 f.). The female statues also reflect the different spirit of the different regions. The Hera offered by Cheramyes in the Heraeum of Samos, dated 570-560, shows a gracious elegance in the Eastern Ionian style (pp. 201 f.), while the Attic Korai from the Acropolis, although dressed in Ionian fashion, are more robust, serene, and self-assured (pp. 263 ff.). The draped figures of a standing young man in Samos and the seated statues of the Sacred Way which led from Miletus to the sanctuary of Apollo at Didyma are semi-oriental in their "lordly relaxation," their immobility, swathed in drapery that hides their flabby forms (pp. 210 ff.).

Karo ends with the "Kritios" boy who embodies Attic personality uniting "Ionian grace and Dorian agility" at the decisive moment of Greek history. \* A revolution in Hellenic and specially Attic temperament is reflected in the artistic change which led to the gravity and seriousness of the Classical Period.

It is certain that in such a comprehensive book opinions are expressed which will not be shared by everybody. The reviewer, for example, does not believe that the Nike by Paionios was made around 425 (p. 223). The Temple of Zeus in Olympia got its acroterion certainly soon after the building was finished, and the well-preserved Nike, carried by the pillar in whose inscription this acroterion is mentioned, belongs according to its style to around 450 B. C. The treasury of Siphnos must not be dated around 525 (p. 232), for the Siphnians lost their wealth in that year, and the rich and elaborate decoration needed a number of years to be finished. Thus 535-525 is a better date. Karo accepts the interpretation of Buschor for the small relief with the quaint device of a building in the center of a poros pediment as Achilles lying in ambush for Troilos (p. 245). I am convinced that it represents a procession in honor of Athena and an old Sanctuary, not a fountain house. The olive tree points to the sanctuary of Pandrosos. The foundations between the Parthenon and the Erechtheum are no more to be considered the Hekatompedon or old temple (p. 246). The investigations of Schuehhardt and Dinsmoor have shown that it is the Peisistratid temple, while the Hekatompedon, to which the well-known groups of the two lions over a bull, the Herakles-Triton, and the "benign triple monster" belong, was probably situated where now the Parthenon stands. The "monster" may be Nereus, the old man of the sea, who can change to water, fire, birds, or snakes, indicated as his attributes. He is dated by Karo "before 560" (p. 248), while the lioness with the bull calf which belongs to the rear pediment of the same temple, is dated about 580 (p. 249). The right date for all these sculptures is about 575-565. The Antenor Kore is dated with Payne too early in 530 (p. 267). Not

only because the marble pediment figures of the Apollo temple in Delphi, dated around 515, are similar, but since after 510 Antenor worked the tyrannicides in Athens, I think that 520 is a better date.

I have found no mistakes, not even in the footnotes, which give well chosen bibliographies. There is some inconsistency in quoting Thieme-Becker, *Künstlerlexikon*, with or without author, with Roman or Arabic numbers. Example p. 317, note 137, where the same reference for Theopropos is given twice in different forms.

An index (pp. 335-343) adds to the usefulness. The price is very moderate. The illustrations are small but excellently reproduced. The selection is good but restricted, probably in order to hold the price down. Karo's wish "an entire volume of plates would have been desirable" (Preface, p. xii) is more than justified. If a volume of plates could be added the usefulness of the book would be enhanced, and it could be used by many more groups of readers who do not have a library at hand.

MARGARETE BIEBER.

COLUMBIA UNIVERSITY.

---

E. A. LOWE. *Codices Latini Antiquiores. A Palaeographical Guide to Latin Manuscripts Prior to the Ninth Century. Part IV: Italy: Perugia—Verona.* Oxford, Clarendon Press; London, Geoffrey Cumberlege, 1947. Pp. xxviii + 40; manuscript facsimiles. \$28.

Professor Lowe's monumental survey of all Latin manuscripts prior to the ninth century has now reached the fourth volume. The enthusiastic praise which the most competent critics have given to previous volumes could hardly be surpassed by anything that might be said about the present one, were it not for the important introduction by which it is prefaced. In this introduction Professor Lowe not only gives a comprehensive critical survey of the manuscript material presented in Volumes I, III and IV—the bulk of what has survived of early Italian book production—but also sets forth, with exemplary lucidity, the principles by which he has been guided in his difficult task. These principles are of the greatest general importance not only for the specializing palaeographer, but for every scholar who has to consult ancient manuscripts. The classical scholar in particular, who counts many of these manuscripts among his most venerable treasures, will find an almost inexhaustible source of information and stimulation in these pages, in which for the first time the manifold problems of our *codices antiquissimi* are discussed by a scholar whose knowledge of early Latin palaeography is unrivalled among contemporaries.

The introduction opens modestly with a partial review of the classification of scripts and their nomenclature. Lowe frankly confesses the difficulty of applying the terms "cursive minuscule" and "Pre-Caroline minuscule" to certain Italian scripts of the seventh and eighth centuries. In fact, he says, it is a matter of degree. Quite different, however, is the case of half-uncial. This term, too, is difficult to define, and Lowe, in his handlist of half-

uncial manuscripts,<sup>1</sup> was wisely content with a "rough rule." Yet it is clearly a script *sui generis*, as Lowe convincingly argues from its definite place in the Turonian "hierarchy of scripts" and the rôle it has played as a model of Insular "majuscule."

These preliminaries are followed by a discussion of some external features which are typical of Italian manuscripts of the early period. Together with the late E. K. Rand, Lowe has been among the first to realize the great practical importance of studying carefully such seemingly irrelevant features as measurements, relation of writing-space to page, pricking, ruling, gathering, numbering of quaternions, besides the more literary feature of spelling. Although Lowe is extremely cautious in his conclusions, he can make some points which open promising perspectives. He has, for example, observed that in most of our earliest classical and biblical manuscripts the writing-space tends to be square, and that in a considerable number of Italian manuscripts from the fifth century onwards the over-all measurements and those of the writing-space are related to each other on the basis of a mean proportional. Another illuminating feature, the *signes de renvoi*, which are merely touched upon here, have been more fully examined by the author in a fundamental article,<sup>2</sup> which, I think, will soon become a classic in palaeographical literature.

Since the pioneer work of Traube and Lindsay, one of the chief tools of the palaeographer has been the study of abbreviations. The Insular abbreviation symbols in particular have challenged the wits of more than one scholar. The fact that Bobbio ranks so prominently among early Italian scriptoria gives Lowe an opportunity to subject Franz Steffen's theory of a Bobbiese origin of these symbols to a penetrating criticism. From a clear analysis of the facts and of the historical probabilities involved the Irish origin of these abbreviations emerges as certain.

Thus prepared, the author attacks the major problem of dating and localizing ancient manuscripts. Its importance for the classical student is obvious; the widely divergent dates that have been assigned for example to the capital manuscripts of Virgil by scholars of fame are only too well remembered. Generations of scholars have labored to work out a chronology of these manuscripts based on objective criteria. The trouble is that there are so few. Yet, as Lowe insists, these are "the bony structure of the whole body of observation on which palaeographical judgments are based." First of all it is necessary to put the available data on a safe footing, all the more so because even these few have sometimes been doubted. Thus Lowe lays down some general principles.

In my opinion the most important one is this: we may safely presume the home and date of a manuscript to be that which is suggested by its continued preservation in a certain place, by traditional connection with a particular person, by subscriptions and corrections, by its contents or other internal evidence, as long as these are not contradicted by the character of its script. Not to presume against the *prima vista* evidence (as has occasionally been

<sup>1</sup> *Miscellanea Francesco Ehrle*, IV (1924), pp. 34-61.

<sup>2</sup> *Miscellanea Giovanni Mercati*, IV (1946), pp. 1-44.

done) is indeed good commonsense—provided that the palaeographical test remains supreme.

By applying this principle to the early Verona manuscripts, Lowe vindicates Traube's opinion that most of them are autochthonous, although positive proof can be furnished only for some limited groups.

More fundamental still is the principle that manuscripts which by a number of common features are distinguished in space and time from other known comparable groups must be approximately of the same date and origin.

To Bobbio alone applies the rule that Italian manuscripts with Insular features, and Insular manuscripts with Italian features, must be products of this scriptorium—the only one of Irish origin and with lasting Irish connections on Italian soil.

It would be very wrong to think that these principles can be applied mechanically. "By a swift and almost unconscious process," Lowe remarks, "subjective impressions are referred to objective data that have been tested and integrated by experience, and from these they derive their authority." This statement is an interesting contribution to the psychology of research—an avowal of the subtle interplay of subjective and objective factors even where there is no room for the arbitrary. Literally the same might be said for example about textual criticism. This attitude is also a welcome reaction to one-sided "objectiveness," which seems to me merely a particular form of the "apathetic fallacy." However, Lowe's method of dating is not a tool for the sorcerer's apprentice. With due allowance for the *imponderabilia*, there must always be a reasoned progress from the known to the unknown. The secret behind Lowe's masterly handling of these instruments is his astounding familiarity with the material, combined with a rare gift of synthesis and a power, if I may say so, of guided intuition. To give just one example: the beautiful uncial of Verona XXVIII (26), Augustine, *De Civitate Dei*, impresses itself at once on the sensitive eye of the palaeographer as a type of great antiquity; the subsequent discovery of a small, but telling detail—a two-letter abbreviation for a *nomen sacrum* (IS for *Iesus*)—confirms the intuitive certainty that this is indeed one of our earliest specimens of Latin uncial. On a larger scale has Lowe's method borne fruit in his list of uncial and half-uncial manuscripts that have a claim to be Bobbiese; this is in fact the first sober list of such manuscripts, as against the indiscriminate attribution of early manuscripts to Bobbio which has so long been in vogue.

In the light of his principles, Lowe then musters the ancient Italian manuscripts in groups according to their script. A date soon before 527 is suggested for the Paris Prudentius in Rustic Capitals (Bibl. Nat. 8084) not only by the famous Mavortius subscription, but also by the contemporary uncial rubries; 494 as *terminus ante quem* for the Mediceus of Virgil rests solely upon the authenticity of the Asterius subscription, but there is no valid reason for doubting it.

Turning to uncial, Lowe refers to his list of "milestones" and his remarks on the dating of this script in *A Sixth Century Fragment of the Letters of Pliny the Younger* (Washington, 1922). In the present introduction he gives merely a list of dated (and partly also localized) uncial manuscripts from Italy. To these he joins a group

that is localized, but not dated; it centres round the Puteanus of Livy, a manuscript that was collated at Avellino near Naples. On palaeographical grounds, its script must be of the fifth century, or perhaps even of the fourth; the same type of uncial is found in all our oldest Livy manuscripts except the Palatinus.

Italian half-uncial is best known from Verona; an important landmark is the Sulpicius Severus written by Ursicinus in 517, and with it can be classed a group of cognate manuscripts. There are similar groups in other centres. Quite a different type, however, is represented by the other famous landmark, the Basilican Hilary, read at Cagliari (Sardinia) in 509/10. It seems unlikely that such perfect half-uncial should have been written in a provincial scriptorium at so early a date. Perhaps the manuscript is African; if it was written in Sardinia, it might be the work of an African scholar who took refuge there from the Vandal persecution. We are reminded of the perplexities of the historical background.

A fascinating script that has been little studied by previous scholars is quarter-uncial. It can be seen in numerous marginal glosses of ancient classical manuscripts; a distinct variety is peculiar to three grammatical manuscripts (*Probus*, *Sacerdos*) which later were at Bobbio. Their script is unmistakably of Italian type; this type of quarter-uncial seems to have "largely inspired the calligraphy of early Irish minuscule." The statement just quoted, which, so far as I know, is made here for the first time, is of great importance. It rectifies the traditional view that Irish minuscule is a mere development of Irish half-uncial, and thus accounts for the traces of cursive in Irish minuscule to which attention has been called by Lindsay.

If from these discussions, which are primarily of interest to the palaeographer, the classical and biblical scholar never returns without benefit, he will be immediately interested in the convincing refutation of the theory, proposed by R. Beer some thirty years ago and still widely accepted on good faith, that a number of the books of Cassiodorus can be traced among the palimpsests of Bobbio. As this detail has been dwelt upon in another review of Lowe's work,<sup>3</sup> I merely refer to it.

The body of the volume follows in arrangement and presentation its predecessors. A change, sure to be welcomed by all students of *Codices Latini Antiquiores*, has been made in so far as manuscripts scattered over several libraries are from now on given their current number at the occurrence of the first fragment, however small this may be.

Needless to say that in the present volume, which comprises a.o. the manuscripts of Ravenna, Rome (except those of the Vatican, surveyed already in Vol. I), Turin, and Verona, the classical scholar finds a noble array of *codices antiquissimi* and *unici* of ancient texts, including a number of famous palimpsests. The description of each of the well over a hundred items, concise and exhaustive at the same time, will amply repay careful study. The Latinist may be specially interested in Lowe's reasoned verdict that the Luxeuil script of Gregory's *Moralia* (nos. 498-501) under which the fifth century Verona fragments of Virgil, Livy, and Euclid are buried must have been

<sup>3</sup> *The (London) Times Literary Supplement*, August 23, 1947.

written either in France or by a French scribe in northern Italy, and that of the original manuscripts only Livy can claim Italian origin.

A highly interesting group is constituted by Turin F. IV. 27 (no. 458), Turin G. V. 37 (no. 464)—both manuscripts of Cyprian—and the famous "African" Old Latin Gospels known as *k* (no. 465). These three manuscripts, once in the Bobbio library, are closely related by their script, a very old uncial, saec. IV-V. The home of these manuscripts is presumably Africa; *k*, however, as tradition holds, was the travelling gospel of the Irish monk Columbanus, the founder of Bobbio. The Irish connections of the *k*-text have been emphasized by H. C. Hoskier, who went so far as to claim the Bobbio Gospels for Ireland against all palaeographical evidence.<sup>4</sup> I observe that Cyprian is the sole ecclesiastical writer whose works have left a trace in the writings of St. Patrick. Did the whole group of manuscripts come from Africa to Bobbio via Ireland?

The bibliography has been compiled with the usual care and thoroughness especially as regards facsimiles, and the reproductions (including most of the palimpsests!) are excellent. The same is true of the letterpress, which reaches a standard of perfection that has become exceedingly rare.

Author and publisher as well as the learned institutions behind them have to be most sincerely congratulated not only on their fine achievement, but also on their courage in continuing this publication amidst the disheartening difficulties of the present times.

LUDWIG BIELER.

UNIVERSITY COLLEGE, DUBLIN.

ENZO V. MARMORALE. *La Questione Petroniana*. Bari, Gius. Laterza e Figli, 1948. Pp. 332. L. 1100. (*Biblioteca di Cultura Moderna*, 444.)

This volume, the result of many years of study, represents a complete reversal of Marmorale's position taken in his earlier works (*Petronio* [Napoli, 1936]; *Petronio nel suo tempo* [Napoli, 1937])—that the Petronius of the *Satyricon* and of the Tacitean account are one and the same. Enrico U. Paoli, in an article "L'età del 'Satyricon'" which appeared in 1937 in *Studi italiani di filologia classica* (n. s. XIV [1937], pp. 2-46), had placed Petronius towards the end of the third century A.D. on the grounds that he saw in *Satyricon*, 70, 10-13 a proof of the *manumissio per mensam*, a practice of a time much later than that of Nero, and on the grounds that Petronius imitated Martial which, as Marmorale shows, is true, but not for the reasons Paoli gives: his arguments could just as well be used to prove that Martial imitated Petronius. Marmorale was irritated at what he believed to be the unsound method used by Paoli and by his belief that Paoli had come to the problem inadequately prepared; and so he published, in the same year, his *Petronio nel suo tempo* in which he attempted to refute Paoli's arguments and to

<sup>4</sup> *Concerning the Genesis of the Versions of the New Testament*, I (1910), pp. 35 ff.

establish the identity of the two Petronii; but now, after a lapse of ten years, he has come to realize that Paoli's conclusion about the date was substantially correct, although not for the reasons Paoli had given. Marmorale, then, in the book under review, makes an exhaustive study of the problem, and comes to the conclusion that the author of the *Satyricon* cannot have been the Petronius of the Tacitean account, but was one of the Petronii who lived after 180 A. D. in the reign of Commodus or, at the latest, in the reign of Heliogabalus.

Marmorale attempts to refute arguments based on the supposed similarity between the character of Petronius as revealed in the Tacitean account and the character of Petronius as disclosed in the *Satyricon*. Nowhere in the *Satyricon*, he insists, do we find anything that might lead to the identification of the two. The argument, often given for identification, that the *luxus eruditus* of the Petronius in the *vita* of Tacitus is in harmony with the tone of the *Satyricon* simply is not valid. Certainly, says Marmorale, the erotic excesses of Quartilla, Circée, and Eumolpus cannot be so characterized. And even if they could, Encolpius, who in the belief of many scholars is the mouthpiece of Petronius himself, finds the *pervigilium Priapi* and Trimalchio's dinner-party thoroughly disgusting.

Marmorale stresses again and again his belief that the coincidence of words, situations, and social usages in any two writers is, in itself, not sufficient grounds for establishing them as contemporaries, for the same argument would hold, as he demonstrates again and again in the case of the *Satyricon*, for periods later than Nero, even later than Commodus, Hadrian, and Heliogabalus. Such evidence, therefore, is valid only when used to support other, more convincing evidence. The appearance of *simplicitas*, for example, in *dicta factaque in speciem simplicitatis* of the Tacitean *vita* and in *Satyricon*, 132, 15, vss. 1-2: *quid me constricta spectatis fronte, Catones, / damnatisque novae simplicitatis opus*, which has been used to support identification, is, in itself, not valid evidence. Again, Tacitus says that Petronius severed his veins at Cumae; but the mere fact that the author of the *Satyricon* shows a familiarity with Campania and that the setting of the *cena* is in a *Graeca urbs* (in all probability *not* Cumae) is, again, insufficient grounds for identification. Tacitus represents Petronius as indulging in songs and frivolous verses with his friends at the time of his death. This, according to some, is what we might have expected of the writer of the *Satyricon*. Marmorale counters with the assertion that nowhere in the *Satyricon* does Petronius describe such a death: on the contrary, the only death scene in the *Satyricon*, that of Lichas, is treated with all seriousness.

The argument for identity, based on the expression *arbiter elegantiarum* in Tacitus and the fact that Petronius is called *Petronius Arbiter* in the MSS, also falls down because, in Tacitus, *arbiter* is an agnomen while, in the case of the author of the *Satyricon*, it is a cognomen. Marmorale does not deny the possibility that the cognomen may have arisen from later association with the Petronius of the court of Nero, but he believes that the identity of the two men cannot be established from the expression *arbiter elegantiarum* and from the name Petronius Arbiter.

After disposing of arguments for identification based on the Tacitean *vita*, Marmorale turns to those based on internal evidence in the *Satyricon* itself. The names of some of the characters in the *Satyricon* are identical with or similar to those of persons of the time of Claudius and Nero. These characters could just as well be identified with persons of the same name at a later time. There is Carpus, for example,—Trimalechio's "Carver." There was a Carpus, a favorite of Nero; but there was also a Carpus, a freedman under Marcus Aurelius, and a M. Aurelius Carpus under Commodus. The fact that Carpus was a common slave-name would, in itself, be sufficient to quash the argument.

G. Studer ("Ueber das Zeitalter des Petronius Arbiter" in *Rh. Mus.*, II [1843], p. 219) identifies C. Pompeius Trimalechio with C. Pompeius Longinus who lived under Claudius; but, as Marmorale points out, Trimalechio could, just as well, be identified with Pompeius Falco, a friend of the Younger Pliny and of Hadrian. That Trimalechio has the agnomen *Maecenatianus* merely indicates that Trimalechio is a "patron of the arts."

Later on, Marmorale takes one proper name after the other from the *Satyricon* and shows that persons with these names lived at a time later than that of Nero. "Echion" is found on inscriptions of a later time at Pozzuoli and at Pompeii; a Norbanus of the time of Domitian is mentioned by Dio Cassius and by Suetonius; a Norbanus was associated with T. Petronius Secundus; "Proculus" is found on several inscriptions from Pompeii and Pozzuoli. There was a Julius Proculus who, along with members of the Petronian family, was put to death by Commodus; there was a Massa of the time of Commodus—and so on.

Again, Marmorale attempts to dispose of arguments based on certain social usages which some scholars have adduced to place the author of the *Satyricon* in the reign of Nero. In many cases he is successful, but in one, at least, he fails. In *Satyricon*, 36, 5 Carpus is described as carving meat, with motions in rhythm with the orchestra, in such a way *ut putares essedarium hydraule cantante pugnare*. Now in Suetonius, *Nero*, 41, Nero is described as entertaining friends shortly before his death by displaying some water-organs of a hitherto unknown sort, and he informs his friends *iam se etiam prolaturum omnia in theatrum*. Some scholars have used these two passages as evidence for placing Petronius in the reign of Nero; but, in Marmorale's opinion, this is not valid evidence because, as he insists (without giving the source of his information), the playing of water-organs at public spectacles belongs to an age later than that of Nero. But Marmorale is apparently unaware of the existence in the British Museum of a coin (or medal) of Nero's time, on which is depicted a small water-organ with a sprig of laurel to the right and, to the left, the victor in one of the public spectacles, with a laurel spray in his right hand. It has been conjectured that this medal was struck in commemoration of the introduction of water-organs at such spectacles. And so, Marmorale is unsuccessful in his attempt to prove that water-organs were used in public spectacles only after the time of Nero, and hence his corollary to this—that Petronius must have lived at a later period—falls down. In any case, the evidence merely shows that water-organs were used at such

spectacles in the time of Nero and *not* that Petronius lived at that time. The mention of *Falernum Opimianum* merely indicates that the wine is "excellent." If Pliny the Younger and Martial, in exaggeration, can speak of "Opimian" wine, why not, says Marmorale, "Opimian" in the time of the Antonines?

Turning to the positive side of his argument for placing Petronius after 180 A. D., Marmorale demonstrates with an impressive list of examples, the stylistic and linguistic unity of the whole of the *Satyricon* and shows that many of the strange forms and much of the strange syntax, contrary to accepted opinion, are common to the educated as well as to the uneducated characters in the *Satyricon*. The *liberti* in the *Satyricon* thus have a linguistic fund in common with the author himself. For example, Encolpius has *usque hoc* for *usque huc*; so Trimalchio says *et hoc et illoc quadrat* and Hermeros, *accede istoc*. Hermeros uses *culcitra*, for *culcita*, but so also does Encolpius.

Marmorale further demonstrates, again with a host of examples, that, while Petronius has been influenced by Seneca and Martial, the supposed anomalies—the use of the diminutive with the force of the diminutive lost, the frequent shifts in gender and in the declension of nouns, the excessive use of personal pronouns, unusual participial forms, the use of the active for deponent forms of verbs and vice versa, adverbs used predicatively, the confusion of state and motion, as in the expressions *in controversiam esse* and *in devorsorio admitti*—are characteristic of the educated as well as the uneducated characters in the *Satyricon* and are part and parcel of the common store of language, not of the time of Nero, but of Apuleius, Fronto, and Marcus Aurelius.

Marmorale is not content to let his argument rest on the basis of stylistic and linguistic conformity of the *Satyricon* with the writings of Apuleius, Fronto, and Marcus Aurelius. He alludes to certain historical facts and social usages to support his argument. For example, he sees, and rightly so, in *Satyricon*, 2, 7: *nuper ventosa istaec et enormis loquacitas Athenas ex Asia commigravit . . .* a reference to the reintroduction of the Asiatic style of oratory and rhetoric into Athens by Herodes Atticus who conducted a famous rhetorical school there and, later, at Rome, under Marcus Aurelius, who was one of his pupils. He contends that the reference cannot be to the period of Nero because of Petronius' use of the adverb *nuper* which means exactly what it says *viz.* "recently." Again, in *Satyricon*, 81, 1 appears the word *antescholanus* (*antescholarius?*), used of Agamemnon's assistant, a word found only in Petronius and in inscriptions which cannot be dated earlier than the first half of the third century A. D. Marmorale supposes, therefore, that the word came into use in the last half of the second century A. D., later fell into disuse, but not until Petronius had made use of it. Mention of the *horti Pompeiani* in *Satyricon*, 53, 5 may refer as reasonably to the *ager Pompei Falconis* of the second century A. D. as to the "gardens of Pompey" or the "gardens at Pompeii." The industry of the *causidici* (*Satyricon*, 46, 7) in the Julio-Claudian dynasty is no proof that the *Satyricon* was written under Nero: *causidici* were just as active under Marcus Aurelius.

This reviewer has merely scratched the surface of the wealth of

evidence which Marmorale has adduced to establish his claim. Not all the evidence is new: Marmorale has made a thorough study of everything that has been written on the subject. He comes to his task with a complete mastery of the literature, language, history, and life of the times. His work is convincing, not only because of certain incontrovertible facts which he uses as evidence for placing Petronius at a date much later than that of Nero, but because of the accumulative effect of his arguments, some of which, to be sure, may be disputed as evidence, when taken independently; but Marmorale has made it clear that he uses such evidence only to support other, sounder evidence to prove his point. Students will find that certain seeming linguistic and social anomalies in the *Satyricon* cease to be such if Petronius is placed at a later date. This reviewer, for example, has always been puzzled by the appearance of the words *sevir* and *seviratus* in the *Satyricon* in a period when the Priests of Augustus were called *Augustales* and when, if inscriptional evidence is worth anything, the term *sevir*, as applied to these priests, was common only at a much later date, as the inscriptions from Ostia indicate.

*La Questione Petroniana* is, in the opinion of this reviewer, the most important study of Petronius that has appeared in recent years. Marmorale's integrity as a scholar and his absolute objectivity of treatment predispose the reader in his favor: he did not set out to prove anything; he wanted the facts to speak for themselves. The facts have convinced him and, I believe, will convince Petronian students, that the author of the *Satyricon* was not the Petronius whose *vita* is given in Tacitus but was one of the Petronii who lived after 180 A. D.

† ELI E. BURRISS.

---

CARL DARLING BUCK. *A Dictionary of Selected Synonyms in the Principal Indo-European Languages*. Chicago, The University of Chicago Press, 1949. Pp. xix + 1515.

Twenty years ago Professor Buck in *Lang.*, V (1929), pp. 215-227 outlined the plan for a dictionary along the lines indicated by the title of the present work, and then, as a sample of the method contemplated, he offered a brief account of the words for *world*, *earth* and *land*, and *sun*. Apart from its purely linguistic interest, a work of this sort can have great value for the history of thought, if close attention is given to the semantic sources of the various words used to express a given idea; and in fact this principle is closely adhered to throughout. That the whole book is executed in masterly fashion will surprise no one who is familiar with Buck's previous work and with his preëminence in Indo-European linguistics. Much of the material in the dictionary will be recognized as the result of his own investigations published in journals during the past few decades, while some portions are based on the studies of pupils working under his direction. The bibliography at the beginning is supplemented by references under the various headings to articles and monographs dealing with words for special groups of

ideas (e. g. Delbrück's *Die indogermanischen Verwandtschaftswörter*, Fournier's *Les verbes "dire" en grec ancien*, etc.).

The compiler of a dictionary of this type is beset by problems more difficult in nature and magnitude than those which meet one whose starting-point is the vocabulary of a given language or, as in the case of Walde-Pokorny, the body of roots which form the working basis for a dictionary of primitive Indo-European. The range of human ideas cannot be exhausted by any known scientific method, nor can they be classified by any simple or infallible system. In his inclusions and exclusions as well as in his classifications the author must sometimes be guided by subjective judgments, with full knowledge that to another person these judgments may at times appear arbitrary, yet it is largely on the basis of inclusion and arrangement that a dictionary of synonyms will be judged. The very size of the Indo-European family is alone enough to make the task a formidable one, and in order to keep the book within reasonable limits Albanian, Armenian, Hittite, and Tocharian are cited very sparingly, and the modern Indic and Iranian languages scarcely at all, but Sanskrit, Avestan, Old Persian, Greek (modern as well as ancient), Latin, and the leading representatives of the Romance, Celtic, Germanic, and Balto-Slavic branches are given liberally. Oscan and Umbrian words whose origins and meanings are well established are generally cited. There are twenty-two major chapters dealing with the physical world in its larger aspects; mankind, sex, age, family relationship; animals; parts of the body, bodily functions and conditions; food and drink, cooking and utensils; clothing, personal adornment and care; dwelling, house, furniture; agriculture, vegetation; miscellaneous physical acts and those pertaining to special arts and crafts, etc.; motion, locomotion, transportation, navigation; possession, property, and commerce; spatial relations: space, form, size; quantity and number; time; sense perception; emotion, temperamental, moral and aesthetic notions; mind, thought; vocal utterance, speech, reading and writing; territorial, social and political divisions; warfare; law; religion and superstition. If some of the matter included in the minor sections appears anachronistic, it must be remembered that this is not a dictionary of Indo-European antiquities, like Schrader's *Reallexicon*, but a dictionary covering the principal Indo-European languages, modern as well as ancient; hence *potato*, *maize*, *tobacco*, *match*, and *gunpowder* are not out of place. In the treatment of animal names it has been necessary to practice fairly rigid economy: names of mammals, both wild and domestic, are given rather freely, with attention to words for the different sexes in domestic animals, where this is important; but apart from the general names for *bird* and *fish* only the names for domestic birds are given. Among numerals only those for one and three are treated in full detail, with full explanation of the various derivative series. Obviously such a treatment of the remaining numerals would be repetitious and unprofitable, since here the inherited words persist with more unbroken regularity (apart from minor phonetic discrepancies) than in any other class. Yet in a work of this sort, having as it does a particular interest for workers in linguistic geography, the inclusion of some of the few known or suspected cases of borrowing among

numerals might have some value (e. g. NE *second* < Lat. *secundus*; Russ. *sorok* < Grk. τεσσαράκοντα; Ch. Sl. (with general Slavic) *súto* presumably from an Iranian source; and also the variation in words for *thousand*).

In arrangement the overlapping of categories and the multiple associations of ideas present some obstacles, as is to be expected. Thus vegetables as well as many specific varieties of vegetables are included in the chapter on food and drink, but *wheat*, *barley*, *rye*, *oats*, *maize*, and *rice* are treated under agriculture. Any difficulty which might arise from this or similar breaking up of congeneric classes is obviated by the index of topics in the back. Of all material perhaps none is so difficult to classify as the great mass of verbs signifying the widest possible variety of acts physical and mental. These have been judiciously distributed among the various chapters, especially those chapters dealing with bodily functions, motion, possession and commerce, spatial relations, sense perception, thought, and speech. The closest approach to a miscellaneous class is Chapter 9, which includes actions connected with a great variety of arts and crafts. Throughout the book each section begins with a listing of the words in the various languages, followed by a general discussion of the semantic sources of the words (e. g. *send* from words for *road* through *go*, *cause to go*, or from *let go*, *throw*, or from *put*, etc., or from *know*, *warn*, *command*). Then follows the more precise etymological description. Where the agreement of several languages gives convincing evidence of a particular word in the parent speech, this word is given first, with its actually known descendants. In a few cases naturally two groups of synonyms both have exceptionally wide currency, as with the words for fire represented by *ignis* and by *πῦρ*. The treatment of the dominant etymological group is followed by a brief discussion of words in Greek, Latin and Romance, Celtic, Germanic, Baltic, Slavic, and Indo-Iranian, generally in this order, but with necessary adaptations to suit the word-distribution in any given case and to keep cognate groups together where possible. In the etymological discussions Walde-Pokorny and the standard etymological dictionaries of the separate languages are liberally cited, but independence of judgment is of course maintained throughout. With the same sober caution that characterizes his other works Buck has given full approval only to those etymological equations which may be regarded as certain, and has relegated many others to a merely probable, conjectural, or otherwise doubtful status. In regard to areal linguistics he briefly explains his somewhat skeptical attitude on page XV. Probably students of this branch of linguistics will constitute one of the most important classes of users of this dictionary. Hence strict objectivity is a matter of the greatest importance, so much so indeed that Buck's apology for letting the facts speak for themselves almost seems unnecessary. This dictionary may be expected to advance the study of linguistic geography through the material assembled in it. At the same time it may exercise a wholesome control over the hazardous speculation which sometimes marks this branch of linguistics. Of individual etymologies only a very few samples can be mentioned here, and these I have chosen exclusively from the classical languages. Buck is slightly less inclined than some scholars to assume Mediterranean sources for troublesome Greek and Latin words. For βασιλεύς and φοῖνος: *vinum* he favors such a source,

but derives θεός < \*θεσός from root \*dhēs-, \*dhəs- (extension of \*dhē- "put"); θάλασσα < \*θαλα-χ-aa, cognate with θάλαμος, θόλος, Goth. *dals*, Ch. Sl. *dolъ*; ἵερός possibly from a pre-Greek form represented by Etr. *aesar* "god," or possibly cognate with Skt. *iśira-*, but he rejects the possibility of contamination of two words. For *populus* he favors connection with πλῆθος; *urbs* with *verbera* (the city being viewed as originally a wickerwork inclosure); *miles* with ὄμηλος, as "member of a crowd, band." For *augur* he prefers connection with *augeo*, *augustus*, to the theory which makes it a compound of *avis*. For *religio* he prefers connection with *legere* to that with *ligāre*, on semantic grounds; for *signum* (against Walde<sup>2</sup>) connection with *in-seque* "say" rather than with *secāre* "cut."

On p. VII read "synesthesia" for "synesthesia" (cf. p. 1019); on p. 80 χθών for χθών; on p. 936 *vienuo-likā* for *vienio-likā*; on p. 1057 "with suffix after the analogy . . ." for "with suffix often the analogy . . ."; on p. 1308 κώμη for κώρη; on p. 1319 *gospodstvovat'* for *gospodstvovat'*; on p. 1321 "accorded" for "according"; on p. 1435 *sūvědětelistvovati* for *sūvědětelistovati*.

JAMES W. POULTNEY.

THE JOHNS HOPKINS UNIVERSITY.

E. DE SAINT-DENIS. *Le vocabulaire des animaux marins en Latin classique*. Paris, Librairie C. Klincksieck, 1947. Pp. xxxii + 120. (*Études et Commentaires*, II.)

This book is essentially what its title implies—a glossary of classical Latin names for aquatic fauna, alphabetically arranged. A typical article cites the Latin name with any variant forms, gives a French equivalent, lists all Latin passages in which the word occurs, gives any ancient Greek equivalent, comments briefly on the etymology, mentions a few Romance survivals of the Latin name, and discusses problems of identity, with quotations from Greek and Latin and modern secondary sources.

The bibliography comprises 32 items (including 3 articles by the author). Of these, 2 are standard encyclopedias, 5 are general ichthyological works by early naturalists, and 5 by recent naturalists, 7 are directly concerned with Latin fish-names and 7 with Greek fish-names, 1 deals with ancient zoology, 2 bear on Ovid's *Halieuticon*, 1 is a general history of fishing, 1 is on Latin etymology, and 1 eludes classification. This phase of the book demands special consideration, because the author's research, although skillfully and intelligently done, was almost entirely confined to these items. Other books and articles infrequently cited in the discussion of specific names do not appear to have been consulted. The deficiencies of the book derive largely from the character of the bibliography, which is limited in scope and haphazard in selection.

For the purposes of the book the following items, to mention only the more important, should have been consulted: Alois Walde, *Lateinisches etymologisches Wörterbuch* (2nd ed., Heidelberg, 1910; 3rd ed., A-L, Heidelberg, 1938); T. G. Tucker, *A Concise Etymological Dictionary of Latin* (Halle, 1931); E. B. Boisacq, *Dictionnaire*

*étymologique de la langue grecque* (3rd ed., Heidelberg and Paris, 1938); J. P. J. M. Brands, *Grieksche Diernamen* (Purmerend, 1935); Reinhold Strömberg, "Studien zur Etymologie und Bildung der griechischen Fischnamen," *Göteborgs Högskolas Arsskrift*, XLIX, No. 2 (Göteborg, 1943); J. V. Carus, *Prodromus faunae mediterraneae* (Stuttgart, 1885-93); *La pesca nei mari e nelle acque interne d'Italia*, Ministerio dell' Agricoltura e delle Foreste (Roma, Istituto Poligrafico dello Stato, 1931); W. Meyer-Luebke, *Romanisches etymologisches Wörterbuch* (3rd ed., Heidelberg, 1935); Walther von Wartburg, *Französisches etymologisches Wörterbuch* (Bonn, 1928-46, plus Beiheft, 1929); Gerhard Rohlfs, *Etymologisches Wörterbuch der unteritalienischen Gräzität* (Halle, 1930); Paul Barbier, fils, "Noms de poissons, notes étymologiques et lexicographiques," *Revue des Langues Romanes*, LI (1908), pp. 385-406; LII (1909), pp. 97-129; LIII (1910), pp. 26-57; LIV (1911), pp. 149-190; LVI (1913), pp. 172-247; LVII (1914), pp. 295-342; LVIII (1915), pp. 270-329; LXIII (1925-26), pp. 1-68; LXV (1927-28), pp. 1-52; LXVII (1933-36), pp. 275-372; Alfred Papendick, *Die Fischnamen im griechisch-lateinischen Glossaren* (Königsberg, 1926); C. D. Badham, *Prose Halieutics* (London, 1854); Francis Day, *The Fishes of Great Britain and Ireland* (1880-84); Arnould Locard, *Histoire des mollusques dans l'antiquité* (Lyon and Paris, 1884); E. von Martens, "Die classischen Conchyliennamen," *Jahreshefte des Vereins für vaterländische Naturkunde in Württemberg*, XVI (1860), pp. 175-284; August Steier, *Aristoteles und Plinius, Studien zur Geschichte der Zoologie* (Würzburg, 1913); H. Schuchardt, "Zu den Fischnamen des Polemius Silvius," *Zeitschrift für romanische Philologie*, XXX (1906), pp. 712-732; Hans Gossen, "Die Tierenamen in Alians 17 Büchern  $\pi\epsilon\rho\iota \xi\omega\tau$ ," *Quellen und Studien zur Geschichte der Naturwissenschaften und der Medizin*, IV, Heft 3 (1935), pp. 128-188; "Die zoologischen Glossen im Lexikon des Hesych," *id.*, VII, Heft 1 (1937). More important than any of those cited above, but unavailable to Saint-Denis at the time of publication, is Sir D'Arey W. Thompson, *A Glossary of Greek Fishes* (London, 1947).

Saint-Denis has listed nearly all the fish-names that occur in classical Latin. The only omissions noted are *scias* (Pliny, *N. H.*, XXXII, 151), *simos* (Pliny, *N. H.*, IX, 23, 30), *capito* (Cato, *Agric.*, 158, 1), and *lagois* (Horace, *Sat.*, II, 2, 22). In general, it is his practice to include only those names actually occurring in classical Latin. He therefore excludes names found only in such authors as Ausonius and Polemius Silvius and those in lemmas, even though they may have been in use in the classical period, as well as names the use of which in the classical era can be inferred from the Romance nomenclature, such as *palumbus* (*palumba*), *aureo* (*aureolus*), *\*mar-suppa*, and *labeo*.

The accuracy of citation of ancient sources is extremely high. Checking of several hundred brought to light only one slight error: Plin. 32.50 for Plin. 32.150 under *apolectum* on p. 7.

Latin source references are very complete and the interpretation nearly always sound. One exception is his taking Ovid's phrase *nostris incognitus oris* (*Hal.* 96) to be an allusion to Italian waters rather than to the northern shores of the Black Sea (p. 46). Greek source references are selective and illustrative, with no pretense at

completeness. Occasionally a key passage important for determining the meaning is overlooked. In the case of the *coracinus* (p. 27), he missed several Greek passages which establish the use of the word as a term for a Black Sea fish, probably the common gray mullet. In some instances a Greek source is misinterpreted. For example, his identification of the *colia* (p. 25) as a sort of tunny (actually the coly mackerel, *Scomber colias* Gmel.) results primarily from misunderstanding of Epicharmus *apud Athenaeus*, VII, 321a.

Inconsistencies are extremely rare. Unusual pains have clearly been taken that statements on the same point in two or more articles should be in harmony.

The author regularly gives a French meaning for a Latin name, but only infrequently cites a scientific name in the ensuing discussion. This often results in ambiguity and obscurity, for the French name in some instances may denote any one of several species, sometimes members of different genera. To mention a single example, *boeuf marin* is hardly an adequate identification of *bos*, even though he mentions the genus *Cephaloptera* (p. 15), for two distinctly different species of *Cephaloptera*, as well as *Notidanus griseus* Cuv., *Heptanchias cinereus* Gmel., and *Zygaena malleus* Val. are all sometimes called "cow-fish" in French. Many of his French terms moreover, are not popular, but learned ones. French *glanis* for *silurus* (p. 104) is only a transliteration of Aristotle's  $\gamma\lambda\alpha\rho\sigma$ , which Pliny translates as *silurus*. This leaves the reader unaware that the  $\gamma\lambda\alpha\rho\sigma$  of Aristotle, like the *silurus* of Pliny, is *Parasilurus aristotelis* Ag., which has no popular name in either French or English.

Saint-Denis' marked proclivity for using superseded Linnaean classifications renders his scientific nomenclature unreliable. Examples are *Perca scriba* L. for *Serranus scriba* C. V. and *Perca cabrilla* L. for *Serranus cabrilla* Cuv. (p. 85), *Sparus annularis* L. for *Sargus annularis* Geoffr. (p. 107), and *Gadus merluccius* L. for *Merluccius vulgaris* Flem. (p. 10). This practice has at times led him into outright errors, such as *Smaris vulgaris* L. (p. 106) (properly C. V.), *Sciaena aquila* L. (p. 22) (properly Risso), and *Umbrina cirrhosa* L. (p. 118) (properly Cuv.).

On the whole, the Romance names are neglected as positive evidence for determination of the meaning of classical Latin names, a result of the limited scope of the bibliography. Examples are definition of *oculata* (*Oblata melanurus* C. V.) as an unknown fish (p. 76), identification of *gerres* (*Smaris vulgaris* C. V.) as a kind of mendole or anchovy (p. 40), and unwarranted suspicion of the authenticity of *garos* as a fish-name (p. 40).

To summarize, the book is a convenient and fairly reliable reference for the general meanings of classical Latin names of aquatic fauna, but it should be shelved with Thompson's *Glossary of Greek Fishes* on one side and a good French dictionary on the other. It is of little value to the ichthyologist who desires precise scientific identifications, and to the philologist who wishes full data on the history of the names. It is especially useful for the completeness of Latin citations, unmatched in any similar work.

ALFRED C. ANDREWS.

UNIVERSITY OF MIAMI.